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ON THE CAUSES, CONSEQUENCES, AND TREATMENT OF INFLAMMATION OF THE VEINS:

WITH EXPERIMENTS ILLUSTRATING THE EFFECTS OF THE MIXTURE OF FOREIGN MATTER WITH THE BLOOD.

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PART I.

I. JOHN HUNTER expressed his belief that the blood has "the power of action within itself",[1] and that when it coagulates, it does so in consequence of an "impression" which it receives. Such an impression may be communicated by separation from the living vessels, or from "cessation of natural action"[2] in them. In certain circumstances also the living vessels themselves may be the means of exciting coagulation.[3] In others, the admixture of extraneous substances may either retard or hasten this operation.[4] The experiments made to determine the last point, Mr. Hunter informs us, "were rather imagined than fully carried out; and the subject rather broached and touched upon, than prosecuted".[5] In these experiments, different articles used in medicine were mixed with portions of blood taken from the body; and it was found that, in some cases, they altered both "the time," and "the firmness of the coagulation".[6] The circumstance of medicines being used in such experiments, conveys the idea, that, in instituting these researches, Hunter conceived that substances which would tend to produce such actions out of the body, might likewise produce some effect upon the blood in living animals. In endeavouring to prosecute the idea thus thrown out, I have been led to try the effect of different substances upon the blood, and to consider the changes which may be produced in that fluid, by the admixture of animal secretions. The experiments which will be hereafter detailed, not only confirm Mr. Hunter's notion, that foreign substances may induce actions in the blood

[1] HUNTER on the Blood. Ed. 1794, p. 21.
[2] P. 25. [3] P. 24. [4] P. 97. [5] P. 94. [6] P. 98.

when withdrawn from the body, but also show that some of these effects may be produced still more rapidly in the living vessels.

In these experiments, pus was used in preference to any other fluid; first, because the power of coagulating the blood which it was found to possess, enables its influence to be traced within the body; and secondly, because, being an animal secretion, the results obtained are likely to be analogous to those produced by the admixture of other secretions with the blood.

When pus is mixed with blood, fresh-drawn from a healthy animal, it is found in a marked manner to favour coagulation. This effect does not take place immediately, as in the case of the mixture of an acid with the blood; and I have reason to believe, that where the blood has lost its natural power of coagulation, no visible change is produced in it by the addition of pus. It appears, therefore, that this effect depends rather upon a vital than a chemical influence. In some cases, the coagulation takes place in less than two minutes; in others, after a longer period; but in all the experiments made, the influence of pus, when added to blood, in promoting its coagulation, was sufficiently evident. Putrid pus was found to act more rapidly than healthy pus (Exp. I, *b*), but the admixture of water was found to retard the operation; the result, in this respect, differing in some degree from the conclusion drawn from a similar experiment performed by Hunter.[1] The causes which usually favour coagulation out of the body, are rest, and separation of the blood into small quantities. These conditions are, in some degree, brought into play during the circulation of the blood through the capillaries; and when the influence of the admixture of pus with the blood is not sufficient to produce coagulation at once, we should naturally expect the effect to be more readily induced, where these two additional causes concur in favouring such an action. When the pus introduced is in any quantity, the coagulation of the blood is at once determined, and the entrance of pus into the circulation thereby prevented. The experiments VI, VII, and VIII, appear to furnish evidence of the correctness of this opinion, and to show that the result may be produced more quickly in the vessels than elsewhere. In these cases, so sudden was the effect, that the mixture of blood and pus coagulated before it could traverse the jugular vein, as indicated by the induration and cord-like feeling of the vessel.

In Experiment VIII, the obstruction formed was sufficient to resist even firm pressure, and in a great measure, if not altogether, to prevent the pus injected from finding its way along the vein. The coagulum was felt in the vessel during the operation, and was there found after death. One effect of the coagulation of the blood thus immediately produced, is necessarily to retain the vitiated blood in the part, and to prevent its being carried in the course of the circulation. This intention may be interfered with, either by accident or design. The coagulum, as in Experiment VI, may be broken up during the process of its formation, or after it has formed, and the parts of which it was composed carried forward with the circulating blood. In such a case, the vein in which the coagulum first formed, is found in its natural condition (except at the part where it may have been mechanically injured),

[1] Op. cit. p. 98.

and dark patches of congestion may be found in distant systems of capillaries. If the coagulum be allowed to remain, the vein in which it is formed soon becomes thickened; but, as the experiments cited prove, *this thickening is the effect and not the cause of the coagulum of blood lodged in the vessel.*

II. When blood coagulates in a serous cavity, a thin pellicle forms upon its surface, and, becoming thickened by deposition from the fibrin of the blood, forms a cyst, which completely circumscribes the effusion. This point has not probably received the attention which it deserves; and as it is believed to be of primary importance in the investigation of the present subject, a short space will be devoted to the purpose of fully establishing it, and tracing its connexion with other and subsequent changes. Every layer of lymph observed upon dissection, has perhaps too generally been considered as the result of inflammation; and hence there has arisen a confusion in the terms employed. That lymph may be derived from the blood directly, and deposited in the form of a membrane, without being secreted by any vessel, has been fully shown by a paper in the *Medico-Chirurgical Transactions*.[1] Such layers of lymph assume so much the appearance of others, derived by secretion from inflamed capillaries, that they have been described as identical. But the mode of their formation in the two instances is altogether different. In the one case, the process is a local one, confined to the blood itself, and subsequently to the membrane with which it happens to be in contact. In the other case, it is an effort of the constitution, accompanied by constitutional symptoms. The former of these processes was clearly described by Hunter. In describing the process of union by the first intention, "Coagulation", he says, "I imagine to proceed upon exactly the same principle as the union by the first intention. It is particle uniting with particle by the attraction of cohesion, which, in the blood, forms a solid; and it is this coagulum uniting to the surrounding parts which forms the union by the first intention: for union by the first intention is no more than the living parts when separated, whether naturally, or by art, forming a reciprocal attraction of cohesion with the intermediate coagulum, which immediately admits of mutual intercourse, and, as it were, one interest."[2] "When the blood has coagulated, so as to adhere to both surfaces and to keep them together, it may be said that union has begun."[3] "The uniting medium becomes immediately a part of ourselves, and the parts not being offended at it, no irritation is produced." "If the quantity of blood extravasated be large, the whole will not become vascular, but the surface only, which is in contact with the surrounding parts."[4] The process thus described in general terms may take place in serous cavities. In the third plate at the end of Mr. Hunter's work, is represented a coagulum of blood adhering to the *tunica vaginalis*. "The adhesion was firm, though it admitted of a separation at one end; when separated, fibres were seen running between it and the testis."

It might seem unnecessary to dwell upon this process further, had not some of the highest authorities in surgery, both here and on the continent, described it as identical with adhesive inflammation. Thus

[1] Vol. x, p. 45-82. [2] Op. cit. p. 26. [3] P. 200. [4] P. 205.

Bichat[1] says, "The cicatrization of wounds in veins after bleeding is a result of inflammation." Now, it is submitted, that when the blood coagulates, either in serous cavities or in veins, the process of union is not usually one of inflammation, or one in which the powers of the constitution are called into increased activity. It is true, that in both cases, inflammation may take place, and lymph, as the result of such inflammation, may be secreted; but this is only when, to use Mr. Hunter's language, the "primary intention" has not been fulfilled.[2]

When a membranous layer of lymph is deposited from effused blood, it adheres with some firmness to the surface with which it is in contact; but, as there is at first no vascular connexion established between them, it may be separated, leaving the part to which it adhered in its natural condition. Lymph derived from adhesive inflammation, on the other hand, when separated, leaves the surface upon which it was formed rough and uneven. Coagulated fibrin, when recently deposited, may thus be distinguished from effused lymph. The changes which blood undergoes when effused in serous cavities, may likewise take place when it is detained in injured or exposed veins. The coagulation of the blood in such cases (Experiments VII and VIII) serves as a bond of union between the sides of the veins (which may be either temporary or permanent), so as to prevent the entrance of any foreign matter into the circulation. When the blood thus coagulates in veins, changes may be produced analogous to those mentioned as occurring in serous cavities. If the quantity of blood be large, a thin pellicle is at first formed upon its surface (see Preparations 1523-25 and 1525-64, in the Museum of Guy's Hospital). This membrane becomes thickened and adheres to the internal surface of the vein (see plate No. 13, Cooper and Travers' *Surgical Essays*, Part I, and Prep. No. 1736, in the Pathological Museum of the College of Surgeons). It then becomes vascular, and finally so firmly united to a part of the circumference of the vessel as to be inseparable from it, without lacerating its lining membrane.

If the wounded vessel be small, or if the animal be strong and robust, the whole of the blood in the vein may at once coagulate and become united to its sides. The usual economy of nature, however, is here exercised, with a precision proportionate to the strength of the patient. A simple wound in a vein, in healing by the first intention, will not obstruct the circulation through the vessel under ordinary circumstances. A coagulum will form, sufficient to unite together the divided edges, and the circulation of blood through the vessel will be uninterrupted; but if the wound does not readily heal, coagula may form, which encroach more or less upon the cavity of the vein. There are then three ways in which a coagulum may obstruct the circulation through a vein. 1. By the outer layer of the coagulum forming a membrane, which contains the more fluid parts of the blood. 2. By the whole of the blood contained in the vessel forming a solid coagulum. 3. By a coagulum adhering to the injured side only of the vessel.

In whichever of these ways the process of repair is commenced, it

[1] Anatomie Générale, vol. ii, p. 423.

[2] In speaking of the two principles, I shall use the term by "first intention", to designate that the fibrin is derived from coagulated blood; and restrict the term "adhesive inflammation" to indicate the effusion of lymph from inflamed vessels.

may be interfered with, and the union dissolved. This is practically known to farriers; who, when they want to bleed a second time from the same orifice, break down the "union by the first intention" by a blow upon the vein. During the time that the parts are united only by the fibrin from the blood, any violence must tend to produce the same effect. If the constitution is good, and the coagulating power of the blood unimpaired, the union may be frequently interrupted, and yet be as frequently re-established in the same way. When from any local cause, or from any constitutional peculiarity, the union by the first intention fails at the seat of the injury, it may yet be attempted at some distance up the vein; and then we have coagula formed at different distances along the vessel. If these coagula fill the vein, are firm, and remain undisturbed by violence, the union may be complete, and the vessel sealed at those parts, even although the original wound should suppurate. But it sometimes happens, that the same peculiarity of constitution, or the same local cause, which prevented the union at the original wound, may prevent complete union by the first intention at any other point of the vein; and then its canal is open to any secretion that may be introduced into it. Foreign matter may thus find its way along a vein; but still there is a provision against its being carried the round of the circulation. It has been already shown that the blood, when in a natural condition, has a tendency to coagulate around pus, and, probably, many other fluids, even out of the body (Experiments I, V), and that this property is exercised in a still more remarkable manner in the living vessels (see Experiments VII, VIII). Foreign matter, even after it has got into the veins, may then, by the same means, be prevented from proceeding farther towards the centre of circulation. The process that takes place under such circumstances, is strictly analogous to union by the first intention. The blood may coagulate and adhere to the sides of any part of the vascular system. The union thus formed may be permanent, or the coagulum may be again broken up and carried with the blood in the course of the circulation, as shown in Experiment VI. When this occurs, as is shown in the same experiment, other changes supervene in remote parts of the body. This tendency to coagulate around the foreign matter once impressed upon the blood, cannot be destroyed by the coagulum being mechanically broken up, as indeed is proved by the fact already mentioned, that after one attempt at union in a vein (in consequence of the introduction of foreign matter) has failed, another attempt is made immediately farther up the vessel. Under these last circumstances, we may find a vein partially obliterated at different points, leaving intervals where lymph or pus are secreted. If the purulent matter introduced is allowed to remain a short time only in the vein, no inflammation is produced (Experiment VI). But when any irritating fluid is detained there in consequence of the blood coagulating around it, adhesive ulcerative, or suppurative inflammation, will be excited (Experiments VII and VIII).

The slowness with which veins inflame when cut, tied, or bruised, has been made a subject of comment by different authors; and Mr. Travers, in particular, has endeavoured to reconcile "the infrequency of its occurrence" with the rapid and violent character of the inflammation in certain cases. Although, under ordinary circumstances, a wounded vein

does not inflame, yet the annexed experiments show, that pus introduced into its cavity will produce inflammation, in which the system will sympathize. Other fluids besides pus will no doubt produce similar effects; but those of pus are here particularly noticed, as affording a good illustration of the series of changes produced by the introduction of foreign matter into the blood.

What the symptoms are which characterize the presence of pus, as distinguished from other secretions in the blood, it would probably be difficult to determine in cases as they occur in practice. The examination of the blood in these instances affords no very satisfactory information; for the characters of pus, when the blood has once coagulated round it, are so altered, that I know of no means by which a small quantity can be recognized, when it has once entered the circulation. The conclusions drawn from the different facts now stated are,—first, that inflammation of a vein, or phlebitis, is no essential part of the primary affection which precedes constitutional symptoms, even when morbid matter has found its way into the circulation through a vein. Secondly, that when inflammation of a vein does occur, in some instances at least, it is not the cause, but the consequence of the introduction of diseased or foreign matter into the blood. Thirdly, that although veins are with difficulty inflamed by any mechanical injury, they are susceptible of rapid inflammation, accompanied with constitutional disorder, whenever any irritating fluids are introduced into their cavities.

III. When the principal veins in a part become obstructed, it is natural to suppose that changes should be produced in the smaller veins which supply them. These changes may be expected in a more marked degree, when the obstruction depends upon coagulation of the blood, than when it arises from other causes, inasmuch as the coagulum usually extends to several veins at the same time.

In the experiments that have been made upon animals, it has been a matter of surprise that, while extreme pain was evinced upon the injection of irritating fluids into the veins, comparatively little or no suffering was produced, when similar experiments were performed upon the arteries. The foreign matter introduced in these cases would probably have the effect of coagulating the blood, as in the instances already mentioned. If this occurred in an artery, the supply of blood below the obstruction would be diminished; but if in a vein, the return of blood would be prevented: in the latter case, the continued influx of blood to the part would necessarily distend the capillaries.

In M. Cruveilhier's[1] experiment, of injecting ink into the veins of dogs, he found, that in thirty-six hours the legs swelled, and a number of bloody patches *(foyers apoplectiques)* were found in the substance of the muscles and the cellular tissues of the limb. The large veins were distended with adherent coagula of blood, and the smaller veins around the livid patches were also filled with coagulated blood. If the animal were allowed to live, the congested spots suppurated. The appearances thus produced in the muscles and cellular tissue of the limb were evidently not those of inflammatory action propagated along the coats of the veins, for the affection in the capillaries was circumscribed, and ter-

[1] CRUVEILHIER'S Path. Liv. XI.

minated in many places abruptly, leaving the veins in the immediate neighbourhood perfectly healthy; still less could the appearance produced depend upon the injected fluid finding its way through the veins (contrary to the course of the circulation) to the capillary system; nor, lastly, could it depend upon the ink finding its way into the general circulation, and producing its effects in its course a second time through the limb; for, not to mention that the capillaries of the lungs and other parts would be equally liable to be affected, one essential condition of the success of the experiment is mentioned to have been, that the fluid injected should *not* find its way along the vein in the usual course of the blood. We therefore conclude, that it was the coagulation of the blood in the large veins which caused the congestion of the capillaries, those veins remaining unaffected which could discharge their contents by some collateral channel. In cases of phlegmasia dolens after child-birth, the same principle can in some instances be clearly traced; thus, in a dissection performed by Mr. Lawrence[1] it is mentioned, that the external and common iliac veins were filled with a substance like the laminated coagulum of an aneurism. "The tube was completely obstructed by this matter, adhering as firmly as the coagulum does in any part of an old aneurismal sac. In its centre was a cavity containing about a teaspoonful of thick fluid of the consistence of pus, of a light brownish red tint, and pultaceous appearance." The femoral vein was in this case also filled with a coagulum; but, as is observed in the account of the dissection, the red colour of that vein might have been caused by the clot everywhere in contact with it, and therefore cannot be deemed a proof of inflammation.

Mr. Guthrie[2] has published a case of inflammation of the veins after amputation, resembling phlegmasia dolens, in which the veins of the opposite limb, even down to the foot, had become affected. In this case, on the fourteenth and fifteenth days after amputation of the right thigh, the left leg began to swell, and became intolerably painful. "The swelling was elastic, yielding to the pressure of the finger, but not in any manner like an œdematous limb. *Upon a careful examination, no pain was felt in the course of the iliac vessels upon that side;* the stump looked well, save at one small point, corresponding to the termination of the femoral vein." On examination after death, the termination of the vein on the surface of the stump *was open*, and in a sloughy condition. At the left groin, the iliac vein was greatly distended with pus. Sir Henry Halford[3] has also mentioned three cases of what he has termed phlegmasia dolens, occurring in the male, in one of which the iliac vein was found obliterated after death. In this case, the patient had suffered, for several years before his death, from swelling of the left leg and thigh. In the interior of the obliterated vessel there is a coagulum, which has lost its colour, and become firm and completely adherent to the inner surface of the vein. (See Prep. No. 1732, Path. Mus. Coll. of Surgeons.) The rapid swelling and general pain of the limb in such cases, indicate a sudden obstruction to the circulation, while the absence of tenderness in the course of the vessels during the first stages

[1] Medico-Chirurgical Transactions, vol. xii.
[2] Medical and Physical Journal, vol. lvi.
[3] London Medical Gazette.

of the disease, tends to show that the contents of the vessels, and not the vessels themselves, are primarily interested in its production.

The foregoing remarks have appeared necessary, in order to explain a circumstance mentioned by Hunter, upon which considerable stress has been laid by subsequent writers. Mr. Hunter observed that the whole side of the head in horses that had been bled would frequently become swollen and inflamed. The explanation of this fact appears very simple, when viewed in relation to the general principle illustrated by the above cases. The horse has only one jugular vein upon each side; and, although in the usual operation of bleeding, its channel is not obstructed, yet if the wound do not readily heal, it contents will coagulate. The circulation will then be obstructed in all the distant branches, and the blood, if long retained, will coagulate in them also. It will then part with its serum, and give rise to all the symptoms of inflammation in the distant vessels; a pulpy elastic swelling, accompanied with great pain, will then be the principal symptom, while the turgescence on the surface will be less than where the superficial veins have been mechanically compressed. It will, however, very frequently happen, that a vein in a part may be felt distended without any symptom of inflammation being present; and, in other cases, the pain and swelling will appear and disappear too rapidly to allow the idea that they depend upon inflammation of the coats of the vein. It has occurred to the author, to feel a vein in the arm and hand distended during life, and after death, to find it empty, and its coats of their natural colour and thickness; in such a case, the coagulum gives way, becomes broken up, and mixed with the circulating blood.

IV. When pus, or other diseased fluid, is confined to the cavity of a vein, the constitutional symptoms produced are comparatively mild, as long as it remains limited and circumscribed by adherent coagula; that is to say, so as to be excluded from the rest of the circulating system. (Compare the frequency of the respiration in Experiments VI and VII.) But the tendency of a clot of blood is to contract; and a time comes when the coagulum is either broken up, or shrinks, so that if no further changes are produced, the current of blood through the vein is re-established.[1] Meanwhile, however, the coats of the veins have undergone changes corresponding to the degree of irritation produced by the contained fluids, and the intention or result to which the inflammation tends. If the coagula have long remained, the coats of the veins are always found thickened, sometimes to three or four times their natural thickness, and sometimes so as to completely obliterate the vessels. The contents of the veins are occasionally found to consist, as far as can be seen, simply of coagulated blood; at other times, they are found filled with soft yellowish coagula, deprived, more or less perfectly, of their colouring matter; more rarely, the cavity of a vein will be found filled with dark-coloured membranous layers, leaving still a channel through the vessel; and occasionally it will be found completely obstructed by "dense, dark-coloured, bluish membranes."

[1] This remark I have had opportunities of verifying in cases, where needles have been introduced under varicose veins in the lower extremities, and allowed to remain, with a ligature around them, for ten days or a fortnight. The circulation through the vein will in such cases be obstructed; but, in a year or two, will be found to have become completely re-established.

As the coagulum contracts in a vein, if the intention is to obliterate the vessel, its sides are gradually approximated. In the smaller veins, and in the divided extremities of large veins, the sides are soon completely drawn together. But the latter, if not wounded, may for a long time (see Prep. 1732, Path. Mus., Coll. of Surg.) retain coagulated blood in their contracted, but not completely closed, cavities. In both cases, the coagula which close the veins are liable to be displaced by accident, or to have their adhesions loosened by the changes which they undergo. The position of a vein, and the structure of the organ through which it passes, may be unfavourable to its healthy reparation. The process of repair goes on frequently during a continued flow of blood over the part, and sometimes during the constant action of the muscles in the neighbourhood: at other times, an injured vein will be situated immediately in the bend of a joint, and will be subject to be continually bent and extended with the motions of the limb. In the structure of the bones, the veins lie in unyielding channels, and are consequently deprived of the assistance derived from the approximation of their sides, as in soft parts, during the process of reparation. As the coagula contract in such a case, there is danger lest the union by the first intention should be disturbed, and that the cavities of the injured veins should be left exposed. Again, in the uncontracted uterus after child-birth, the veins which open upon the placental surface, pass through the firm texture of the organ, and are incapable of contraction independently of the muscular structure which surrounds them. The coagula which close their extremities secure them against the entrance of any foreign matter; but should these coagula be removed before the vessels are otherwise protected, their open mouths are exposed to any secretions that the uterus may happen to contain. In these cases, if a coagulum is not firmly formed, or if it is displaced by violence, it may be broken up, and portions of it mixed with the fluid blood. Subsequent coagula may form in the veins and offer fresh obstructions to the admission of any foreign matter, but these may, as in the first instance, be disturbed, and carried, together with any admixture of the secretions of the part, in the course of the circulation. The period at which the union of a coagulum in a vein is dissolved, is sometimes marked with great precision. In a case recorded by Dr. Davis,[1] a patient was convalescent from an attack of phlegmasia dolens, when death took place instantaneously, while the patient was in the act of changing the sitting for the recumbent posture; the left external iliac vein was thickened, and its internal tunic was studded in several places with deposits of adherent lymph. The portion most remarkable for this incrustation, as well as for other disease, was immediately beneath Poupart's ligament; the vein, although contracted, was *manifestly pervious.*

V. It has been shown in the previous sections, that secretions mixed with the blood will alter its properties, and influence the period of its coagulation: that when the blood is thus altered, it may pass through a vessel without leaving any trace of its passage; but that if it coagulates and remains in a vein, the coats of the vessel will then take on increased action. The exciting cause of the inflammation in such cases appears to be conveyed by means of the contents of the vessels to the

[1] Medico-Chirurgical Transactions, vol. xii.

vessels themselves. But, as in post-mortem examinations, the changes produced in the vessels are much more easily recognized than the alterations in their contents, the former have of late years almost exclusively occupied the attention of pathologists. The cases in which constitutional symptoms follow inflammation of the veins, will be found to divide themselves principally into three large classes. 1. Those in which one of the larger veins has been opened. 2. Those in which some portion of bone has been involved in the original lesion. 3. Those that occur after child-birth.

In each of these three classes of cases, a free communication will be found to exist between the injured part and the general circulation. The natural mode of sealing this communication, when it is no longer proper, is the coagulation of the blood in the veins of the injured part. When, from some constitutional affection, or from some local peculiarity of structure, this intention is not fulfilled, a ready passage remains open, through which the blood may become infected. In injecting fluids into the veins, it has frequently happened, that no great constitutional disturbance, and no signs of secondary inflammation, have been produced; but this is believed to have depended upon the coagula in the veins having prevented (as probably occurred in Experiments VII and VIII) the foreign matter from finding its way along the vessels. But if this obstruction be not offered, or be overcome, then the appearance of secondary inflammation, accompanied by corresponding constitutional symptoms, will be produced. If water be injected into the cancellous structure of bone, it will find its way out in drops through the apertures of the nutritious vessels. The ready communication which is thus shown to exist between the interior of bones and the veins, has been but too often exemplified by M. Cruveilhier's experiments of introducing mercury into their cancellous structure, and finding it subsequently in the vascular system. This fact assumes peculiar significance, when taken in conjunction with the very large proportion of cases, in which some portions of bone will be found to have been involved in the primary lesion, in those who have died of secondary inflammations. Of fifty-two consecutive cases, occurring in surgical hospital practice, of which I have preserved notes, in no less than forty-one was some portion of the osseous system implicated. Again, in the third class of cases, if the vena cava be injected after parturition, the injection will very speedily find its way into the uterus.[1] The ready communication which is thus shown to exist between the vascular system and the local affection, in each of the three large classes of cases which usually give rise to subsequent disease, would of itself afford at least a very remarkable coincidence. But more direct evidence presents itself of the way in which the system becomes contaminated in these affections: thus, after an operation for hæmorrhoidal tumours, an effusion of lymph and pus has been found in the hæmorrhoidal veins,[2] from thence the same appear-

[1] DANCE. Archives Générales de Méd. vol. xviii, p. 480, Dec. 1828.

[2] In cases where pus has been found in veins surrounded by coagula, its presence and detention there have been differently accounted for. M. Cruveilhier appears to have imagined that the loose coagula act as filters, through which the blood passes, while the pus is retained. (*Dict. de Med. et de Chir.* t. xii, p. 641.) The true explanation of the way in which coagula form round pus in the veins has already been given.

ances have been traced to the inferior mesenteric vein, and the severity of the secondary affection, indicated both by the symptoms and the post-mortem appearances, has fallen upon the liver. These circumstances all tend to point to the venous system as the means by which morbid matter in such instances is introduced: and the still more conclusive facts afforded in the production of secondary disease, by injecting fluids into the veins,[1] allow scarcely a doubt to remain upon the mind, that the unprotected veins are the channels, in a very large proportion of cases, through which the blood becomes infected.

VI. The cancellous structure of bone may be compared to the cellular tissue in soft parts. When inflamed, its intervals become filled up by effusion from the vessels, and an abscess may be as accurately circumscribed in the hard as in the soft structures of the body. In a healthy constitution, the adhesive inflammation will always precede the suppurative; but where the inflammation is not circumscribed by adhesion, the secretions may permeate from cell to cell in unadhering parts. In soft structures, a remedy is at hand for allowing the escape of the secreted fluid, by a free division of the parts; but in bone, where the same thing takes place, the hard unyielding sides offer an effectual obstruction to the escape of any effused fluid. The cells of the bone then become infiltrated, and the same want of power which is manifested in the deficient adhesion in the bone, will probably be indicated by want of union in the veins. There is nothing, then, to prevent any secretion permeating the cancellous structure from finding its way into the circulation.

M. Cruveilhier assures us, that a single drop of mercury introduced into the cancellous structure of living bone, may subsequently be detected in the capillaries of the lung, where it becomes the centre of one or more patches of livid congestion. This experiment appears to afford a perfect illustration of the way in which diseased secretion may be conveyed into the circulation, when the natural processes of repair in bone are abortive. These processes, as we have seen, are,—first, union by the first intention; secondly, adhesive inflammation. In soft parts, as the fibrin, which forms the bond of union in the first of these, is absorbed, the divided veins collapse, and thus continue closed; but in bony structures, where the injured vessels are held open, as the fibrin which at first closed their extremities becomes removed, their channels may be left as much open to the diseased secretions of the part, as to the globule of mercury in M. Cruveilhier's experiment.

The low degree of organization in bone, and the comparative slowness with which actions are there carried on, render it, in a peculiar degree, liable to interruptions in the process of repair; especially when, as not unfrequently happens, there is reason to believe that the vitality of some portion of the bone has been threatened. The offensive smell of the bone, as well as the appearance of its cancellous structure infiltrated with puriform matter, will frequently show in such cases, that the processes above-named have not followed their natural course.

VII. As a necessary deduction from the accompanying experiments, and those of M. Cruveilhier, alluded to in the previous section, we

[1] See M. Gaspard's experiments.

arrive at the conclusion, that a vitiated condition of the blood may give rise to inflammation of the veins in different parts of the body. The circumstances which occasionally attend reparation of the uterine veins after child-birth, will be found to lead to the same inference; and the same general proposition will derive fresh support from the consideration of this class of cases.

The veins which terminate upon the placental surface of the uterus are necessarily open when this organ is distended, and become more or less perfectly closed when it contracts. In cases when the contraction is incomplete, innumerable open-mouthed orifices are left bathed in secretions, which are often offensive and undergoing decomposition; the natural protection to the vessels then, is the coagulation of the blood in them. If examined, the uterine veins will be found filled with coagula for some distance. But in cases where this power is impaired, all the uterine veins and arteries recently separated from the placenta may be found bathed in the secretions of the part, under circumstances most favourable for their absorption. The passage of diseased secretions through the vessels cannot always be traced in this, any more than in the other forms of the disease. Many of the substances introduced artificially into the circulation by M. Gaspard, produced no action upon the coats of the veins through which they passed, and yet the general symptoms were precisely similar to those originating from genuine phlebitis. In accordance with this, it may be observed that the uterine veins are often found perfectly healthy when the spermatic, or renal, or still more distant veins are thoroughly disorganized. In either case, the healthy condition of the veins near the original lesion forbids the idea of inflammation having been propagated along the coats of the vessels, while all analogy appears in favour of the disease being transmitted through their contents.

In a certain number of cases no lesion will be found in any of the veins of the body, but the uterine veins will be found to contain some unnatural fluid; at other times coagula of blood, which have lost their elasticity, gritty to the feel, and greyish or light brown in appearance, will be found filling the veins or leaving intervals in them, where lymph or pus may be recognized. It matters little whether the unnatural fluids, thus found in the uterine vessels, have been absorbed from the cavity of the uterus, or are the product of venous inflammation. The effect upon the blood in either case would be the same.

When obstructions form in the spermatic veins, they are not indicated by any external symptoms; but when the veins opening into the internal iliac are similarly affected, the coagula are liable to extend into its cavity, and even beyond it to the external and common iliac vessels. The free return of the blood from the inferior extremity, will then be prevented. The effects of this have already been described (sec. III.)

The connection of this form of disease with affections in distant parts of the body, has been noticed by several eminent writers. Legallois has expressed his conviction, that phlegmasia dolens, puerperal fever, and many other puerperal ailments, are solely dependent upon the absorption of pus from the uterine surface. This opinion appears to have been formed upon too hasty a generalization, inasmuch as other fluids besides pus, as evinced by some of the annexed experiments, may pro-

duce similar effects upon the blood. But that pus, when absorbed, will determine the coagulation of the blood in the iliac as well as in other veins, must be allowed; and that the symptoms of obstructed venous circulation arising from this cause, will exactly resemble those of phlegmasia dolens, will scarcely be denied.

"Besides depositions of pus in certain portions of the frame," observes Dr. Ferguson, "I have seen two other states of the limb, which are connected with and traceable to the cause originating puerperal fever. In one of these the malady looks like erysipelas . . . .; in the other, the leg is attacked with a disease so exactly resembling phlegmasia dolens, as to leave no doubt in my mind that they are one and the same malady. In this, as in other forms of the disease, there may be a tendency to gangrene of the skin."

The period of the occurrence of what has been described under the name of uterine phlebitis is marked with much precision, and the affection of the system is often general and sudden. It may be stated as the result of all the observations hitherto made, that it occurs most frequently from the 10th to the 20th day after parturition.[1] If the inflammation in such cases were propagated along the vessel only, it would be difficult to account for such an apparently capricious selection of time for its development. This difficulty, however, disappears when the period is observed to be so strictly in accordance with the time at which the same symptoms occur after other local complaints, and to be, moreover, the time at which the coagula formed in the veins, may naturally be expected to shrink.

It has been observed, that inflammation after child-birth usually attacks the spermatic veins alone, and for the most part the one only on that side of the uterus to which the placenta has been attached. The hypogastric veins are comparatively rarely affected. The appearances observed upon dissection in the spermatic vein, usually terminate abruptly at its opening into the vena cava on the right side, or into the renal on the left. This fact is in perfect accordance with that observed by Mr. Arnott, that the coagulum in veins extends usually only to the nearest collateral branch; the explanation appears to be the same in both cases, as illustrated by Experiment VI. If the coagulating blood be left undisturbed, it will form adhesions to the sides of the vessel and produce increased action in its coats; but if mechanically disturbed, it will be carried forward before the process of coagulation is completed, and leave the vein in its natural condition. When any portion of a vein is obstructed, the blood is kept at rest between the obstruction and the next collateral branch; and, if disposed to coagulate, there is nothing to interfere with such an action. But the case is different, as soon as one vein opens into another. A fresh current of blood is then continually sweeping the orifice of the obstructed vessel; and, even although the blood at this point should have a tendency to coagulate, it is carried on in the course of the circulation, before it can adhere to the sides of the unobstructed vein. The sudden termination of the diseased appearances in these cases, affords an additional proof that the blood is the medium by means of which this affection is transmitted. It is true, in

[1] Dr. Lee. Medico-Chirurgical Transactions.

such instances the diseased fluid cannot be always, or even generally, traced in the veins, and very many cases occur where a retained and putrid placenta, or decomposing coagula, remain in contact with the mouths of the uterine veins, without any of the symptoms of local phlebitis being produced; but this is only in accordance with what is observed in cases where purulent or other fluids have been directly injected into the blood. The examination of the blood, or of the vessels, in such cases, will by no means invariably indicate the presence of foreign matter after it has once become thoroughly mixed with the blood, nor will inflammation of the vein through which the fluid passes, be by any means invariably produced.

When a foreign substance is introduced into an artery, any immediate effects upon the blood may naturally be looked for in the system of capillaries which it supplies. If the blood then coagulates, local symptoms alone, will, in the first instance, be produced, and the constitution will remain unaffected. M. Magendie,[1] indeed, asserts that fluids injected into the arteries of animals, return quickly through the corresponding veins, and that this takes place even more rapidly in the living than in the dead body. If this were universally true, it would matter little whether foreign matter were introduced into the arterial or venous system. The effect upon the constitution would be the same in either case. But if, as is now maintained, extraneous matter introduced into the blood may, under certain circumstances, produce its coagulation, then the effects will be confined, more or less completely, to the first system of capillaries which the blood meets with in the natural course of its circulation, and the constitution will be affected only in consequence of the changes which then take place. M. Gaspard has shown that greasy fluids, and such as contain sediments, do not find their way readily from the small arteries into the veins. They become entangled in the intermediate capillaries, and there produce, first patches of local congestion, and subsequently serous effusion and abscesses. Some clear fluids, on the other hand, such as solutions of tartar emetic, of opium, and of nux vomica, when introduced into an artery, pass readily in the course of the circulation, and produce their full effect upon the constitution; and in such cases no irritation is manifested in the capillaries through which they pass. The first of these poisons produces vomiting and purging, the second stupor, and the third tetanic rigidity, exactly in the same manner as if they had been introduced into the stomach, or injected into a vein.

There are yet another class of substances differing in their effects from both of the former; and under this head are classed infusion of tobacco, solution of acetate of lead, putrid fluids, etc. These are distinguished from the first class above mentioned, as not offering in themselves any mechanical impediment to the circulation of the blood, and from the second, as not producing the same constitutional symptoms when injected into an artery as when thrown into a vein. M. Gaspard found that, when introduced into an artery, the infusion of tobacco neither produced vomiting nor stupor, the solution of acetate of lead did not act upon the intestines, and the putrid fluids did not produce the evacua-

[1] Précis Elémentaire de Physiologie, t. ii, p. 389.

tions usually observed after their introduction into the system by other means. All these substances, however, were found to produce violent local irritation in the parts to which the branches of the injected artery were distributed, and the constitutional symptoms were those produced in consequence of the local irritation, and not those which would arise directly from the action of those poisons upon the system.

In Experiment xx, seven or eight cubic inches of common air were gradually injected into the carotid artery of a dog, and half an hour afterwards an ounce of water, to which seventy drops of medicinal prussic acid had been added, was thrown into the same vessel; none of the peculiar effects of the poison followed this operation. At the expiration of another quarter of an hour, an ounce of a saturated solution of nux vomica was likewise injected, still without producing any constitutional symptoms. It is very remarkable in this experiment, that M. Gaspard[1] should have considered that the elasticity of the air contained in the vessels was sufficient to counteract the impetus of the blood, and thus to prevent the progress of the poison along the vessels, especially when we find him stating that, on a post-mortem examination, the smaller vessels appeared to have been *obstructed by very hard clots of blood.*

EXPERIMENTS.

I *(a)*. On the 25th of September, 1848, having procured four small vessels of equal sizes, I placed in the first some dilute sulphuric acid, in the second some offensive pus, and in the third some water. The fourth vessel was left empty. They were then all equally warmed, and some blood from the jugular vein of a healthy horse was received into each of them so as to fill them to the same level. They were now stirred with separate pieces of wood. At the expiration of two minutes (noted by a watch), the contents of the second vessel had become coagulated into one uniform mass. The contents of the first vessel (containing the acid) were thickened and of a dark brown colour; in the third and fourth cups the blood was of its natural fluidity, but darker coloured in the cup containing water than in the other. At the expiration of ten minutes, the blood contained in the fourth cup had begun to coagulate; the blood and water still remained fluid. At the expiration of a quarter of an hour, the blood had completely coagulated in the fourth cup, containing blood alone; and had very partially coagulated in the third cup containing the blood and water.

(b). Four vessels were taken, each capable of holding three fluid ounces. In the first was placed half an ounce of cold water, in the second half an ounce of dilute sulphuric acid, and in the third half a drachm of pus, which was quite fresh and sweet. All the vessels were then quickly filled with blood, from the jugular vein of a horse. The contents of each vessel were stirred. The blood and dilute sulphuric acid became thick, and changed in colour almost immediately, as in the first experiment, but did not coagulate. The pus and blood coagulated in six minutes, and the mass was firm in seven. The pure blood coagulated in twelve minutes and was firm in sixteen. The blood and water

[1] Journal de Physiologie, t. v, p. 328 and 336.

coagulated in about the same time, but took nineteen minutes to become firm.

The above and the following experiments were made at the suggestion of the author, in conjunction with Mr. T. W. Mayer,[1] veterinary surgeon.

II. An abscess was opened in the groin and a quantity of pus received into a gallipot; some blood from the divided vessels was also received into the same vessel; they were then stirred together, and in two minutes the mass coagulated. Some blood taken from the same patient in the same manner, but not mixed with pus, coagulated in eleven and a half minutes.

III. On the 20th of January, 1849, an inflamed and suppurating abscess was opened, and the blood and pus which flowed from it were mixed together. They coagulated in two minutes and twenty seconds. This experiment was repeated several times, with nearly similar results.

IV. In June 1849, a tense inflamed swelling was opened in the perinæum of a patient, who had for years laboured under a very obstinate stricture. A quantity of matter first escaped, and subsequently serum, mixed with shreds of lymph and small quantities of pus and blood, continued to flow for some time. Portions of this mixed fluid were received into separate vessels; they coagulated on an average in about two minutes.

V. Two ounces and six drachms of blood were taken from a healthy horse, and two drachms of pus were mixed with it. The mass coagulated in three minutes and three-quarters.

VI. A healthy male ass, three years old, was procured, and, with the assistance of Mr. Mayer, was made the subject of the following experiment, on the 23rd of September, 1848. Three drachms of pus were collected from an issue in the chest of a horse, which laboured under inflammation of the lungs. The pus thus obtained was quite pure and sweet, and having been warmed, was injected, by means of a syringe, into the left brachial vein of the ass. The animal lay quiet, till nearly the whole of the pus was injected; it then struggled, and a small quantity of the pus may have been lost. When the operation was completed, the sides of the vein were brought together with a pin, and the animal was allowed to get up. The vein above the opening could now be felt as a hard, unyielding cord, as high as it could be traced with the hand; but upon gentle pressure being made, so as to propel the blood in the course of the circulation, the hardness completely disappeared. The vein which, immediately after the operation, was hard and prominent, no longer presented anything remarkable to the touch. The animal now moved from side to side, as if inclined to lie down.

Two hours and a half after the operation, the pulse, which naturally was 36, had risen to 60; and the respiration from 12 per minute had increased to 26.

September 24th. Pulse 52; respiration 20; mouth hot; ears cold. In the evening the pulse became 48 and the respiration 16; he coughed occasionally.

25th. Pulse 48; respiration 12; some dulness of countenance, but

[1] The present mayor of Newcastle-under-Lyme.

he is lively and occasionally playful. The left fore-leg is swollen; the ears are very cold. In the afternoon he was killed, and the blood was allowed to flow from the body.

Post-mortem appearances. The wound in the left leg opened directly into the brachial vein, which was filled with lymph and a thin pus for a very short distance, both above and below the external opening; immediately above this, the vein was healthy, nor was there any appearance of disease in any of the other veins of the limb, nor in the veins leading to the heart. The glands in the axilla were swollen. The lungs were found studded irregularly in different parts, with circumscribed spots of livid congestion: these existed both upon the surface and in the substance of the lungs; they were generally about the size of a filbert, but in some places they occupied a single lobule, and were accurately circumscribed by its outline.

VII. On the 23rd of November, 1848, about an ounce of perfectly pure pus (previously warmed) was injected into the right jugular vein of an aged ass; the vein immediately became "corded", and the blood appeared to have coagulated in the vessel. The operation did not much excite the breathing; but the pulse, which naturally was 35 in the minute, rose to 60, and subsequently fell to 55.

24*th*. The animal dejected; appetite indifferent. The vein can be traced as a thickened cord as far as the sternum. Respiration 12 (the natural standard); pulse 50.

25*th*. The parts around the vein much infiltrated with serum: pulse 55; respiration 12.

26*th*. The wound in the neck began to suppurate, and an abscess subsequently formed in the course of the vein, about midway between the opening and the sternum. The general symptoms continued, with very slight variation, until the 4th of December, when the animal was destroyed.

Post-mortem appearances. The jugular vein was found to have become inflamed only in the course of the circulation, and to be obliterated a short distance below the external opening. The surrounding parts were greatly infiltrated with serum and lymph, and several abscesses had formed in the immediate neighbourhood. The lungs did not present any well-defined patches of congestion, as in the last mentioned experiment.

VIII. A healthy ass, six years old, was operated on upon the 16th of November, 1848. The respiration was naturally 14 in the minute, and the pulse 38. About two ounces of highly offensive pus, obtained from the frontal sinus of a horse, were injected into the left jugular vein; the pus had unintentionally been mixed with water previous to its being injected. The vein became full during the operation, as though the blood in it were in a semi-coagulated state. The pulse now became 60, and the respiration 20 in a minute; slight rigors occurred in two hours.

November 17*th*. The animal is tranquil; appetite good; pulse 48, small and wiry; respiration 16. In the evening he was rather more excited; the vein was becoming inflamed downwards towards the heart; pulse 60; respiration 20.

November 18*th*. The vein was more inflamed, and slight suppuration was visible at the orifice of the wound. Respiration 16; pulse 55. From

this period to the 23rd, the pulse continued from 55 to 60, and the respiration varied from 12 to 18.

November 26th. The swelling in the situation of the vein is rapidly subsiding; pulse 55; respiration 12.

The animal gradually recovered, and on the 26th of February, 1849, was made the subject of another experiment. The right jugular vein having been opened, two fluid ounces of pure healthy pus were injected, and propelled in the course of the circulation, by pressure upon the vein externally. The vein became tense during the operation, and sensibly resisted the attempts that were made to propel its contents towards the heart. *Even forcible pressure was not sufficient to overcome the resistance offered to the return of blood.* Soon after the operation, the animal had a rigor; the breathing became laborious, but not accelerated; pulse 57.

After the lapse of seven hours, the animal appeared dejected; he refused to eat or drink; the extremities were cold; breathing 16 in the minute; pulse 60, small and irregular.

February 27th. The vein can be felt thickened as far as the sternum. The general symptoms are the same as on the previous evening.

28th. There appears less constitutional irritation; pulse 60; respiration 14.

March 2nd. Appetite still indifferent; pulse 60; respiration 16.

From this date to the 7th, when the animal was destroyed, the general symptoms continued much the same, but the induration and swelling around the jugular vein, from the opening to the sternum, became greater.

Post-mortem appearances. The left jugular vein was found completely obliterated. The remains of a firm coagulum obstructed its canal for some distance below the opening which had been made into it, and terminated, below, in an elongated conical portion, which adhered to one side only of the vessel. On the right side, an abscess had formed in the course of the vein; and for two inches, thew hole of the parts were imbedded in a confused mass of pus and lymph, in which it was impossible to distinguish the structure of the vein. Both above and below this, for several inches, the vein was filled with coagula, which effectually obliterated it. These coagula extended for several inches in the course of the circulation; but beyond them, in both directions, the vessel was pervious. The lungs presented some slight spots of congestion, but not of the same characteristic kind observed in Experiment VI. The other organs were healthy.

IX.[1] Two drachms of pus, somewhat fetid, derived from a large common ulcer, and diluted with a little water, were injected into the jugular vein of a middling-sized dog. The animal immediately made several convulsive efforts to swallow, and soon became faint. It showed indications of pain, and vomited more than six times in the course of the day. At the expiration of an hour, it appeared slightly relieved by an evacuation, and by passing turbid urine. In the evening, it was very ill; it lay upon its side with its legs extended; had a very feeble pulse and scarcely perceptible respiration. Ten hours after the experiment, it

[1] This, and the following experiments, are among those recorded by M. Gaspard, referred to in the foregoing dissertation.

passed black, liquid, and extremely offensive motions; these were accompanied by immediate relief. The animal regained its appetite, eat and drank freely, and went to sleep. The day following, it appeared nearly well. On the third day, three drachms of the same pus were injected into the opposite vein; after the lapse of a certain time, there occurred, as in the first instance, faintness, vomiting, and frequent desire to pass urine; twelve hours after the injection, frequent liquid, white, and very fetid motions were passed, and it died at the expiration of twenty-four hours. On opening the body, no alteration was found either in the intestines or other organs.

X. The last experiment was repeated on a greyhound with the same results: faintness, fever, vomiting, and repeated evacuations succeeded each other, with recovery after the first experiment, but not after the second. On opening the body, no lesion was observed, except that the inferior lobes of the lungs were gorged and almost hepatized.

XI. Three drachms of recent pus, derived from the same patient as in the last experiments, were injected into the jugular vein of a small emaciated unhealthy dog. After the expiration of three minutes, there was an abundant evacuation of urine, followed by repeated vomiting, and repeated ineffectual efforts to pass fæces. For nearly a quarter of an hour, there was a kind of emprosthotonos, rigidity of the limbs, and a death-like condition. Subsequently, fresh vomiting ensued, with very fetid liquid evacuations, which were followed by apparent relief; soon after, however, long continued tenesmus made its appearance, and terminated in death, five hours after the injection of the pus. On opening the body, the mucous membrane of the intestines was found red, swollen, and inflamed, especially in the colon and rectum.

XII. Half an ounce of pus, similar to that used in the preceding instances, but more putrid, in consequence of having been longer kept, was introduced into the veins of a middling sized dog. The animal, as in the other cases, was seized with vomiting, accompanied by violent straining. Subsequently, strongly marked nervous symptoms made their appearance. The eyes wandered; there was extreme sensibility, and involuntary convulsive twitching over the whole body, accompanied by faintness, hiccough, and short piteous cries. The walk was unsteady, staggering, and without apparent object. There was furious delirium, ardent thirst, dyspnœa, palpitation of the heart, etc. This state lasted for nearly two hours, and the animal died in frightful convulsions, without having experienced any critical evacuations, as in the former cases.

Post-mortem appearances. On opening the body, while still warm, the venous blood was found very firmly coagulated, not parting with any of its serum when left at rest; the left ventricle of the heart showed, on its external surface, some stains of the colour of lees of wine, formed by a kind of concrete pellicle, which disappeared only after long rubbing and maceration. The other organs appeared healthy.

XIII. Some beef was allowed to decompose in some dog's blood; half an ounce of the fluid resulting from the decomposition, was injected into the jugular vein of a little bitch. Immediately, the animal made several convulsive efforts to swallow, and soon became oppressed, uneasy, and faint. At the expiration of an hour, there was great prostration, accompanied by repeated gelatinous and bloody evacuations, and vomiting of

bilious matter. The strength became gradually less, and the animal died three hours after the injection.

Post-mortem appearances. The lungs were found inflamed in a very peculiar manner. They were gorged with blood, of a violet or black colour, and presented many petechial spots, like small ecchymoses. These spots existed also on the left ventricle of the heart, in the spleen, in the mesenteric glands, in the gall-bladder, and even in the subcutaneous cellular tissue. The peritoneum contained some spoonsful of a reddish serum; but the mucous membrane of the digestive organs was found to have been principally affected. In the stomach it was slightly inflamed; in the intestines, but especially in the duodenum and rectum, it was of a livid colour, presenting many black spots, and covered by a gelatinous and bloody secretion, resembling lees of wine. The tissues in these parts were slightly thickened.

XIV. The preceding experiment was repeated, by injecting into the jugular vein of a moderately large dog, an ounce of fluid, derived from the maceration of putrid beef in water. The animal very soon passed extremely offensive, liquid evacuations, with much urine. The breathing became quick and deep, the pulse small and quick. Repeated efforts were made to empty the bowels. There was great depression and want of strength. At the expiration of an hour, a kind of diarrhœa or dysentery made its appearance. Liquid, bloody, and fetid evacuations, continued for an hour and a half, when the animal died.

Post-mortem appearances. Livid, brown, and black patches were found scattered over the lungs. The intestinal canal was filled with a bloody mucous secretion, resembling the matter that had been voided; its mucous membrane was of a livid colour, as in the preceding case.

XV. Two ounces and a half of thick fetid fluid, derived from the maceration of cabbage leaves in an equal quantity of water, for two days, at a temperature of 77 Fah., were injected into the right jugular vein of a moderate sized dog. During the operation, the animal made several efforts to swallow, and soon became faint, and vomited several times. Some hours afterwards, there was great uneasiness and oppression, with recurrence of the vomiting, and continued faintness during the day. After nine hours, a most copious and very fetid evacuation took place. The discharge was as black as soot, and composed of mucus, with a little fæcal matter, and a large quantity of what appeared to be corrupted blood. Some time afterwards, there was a second evacuation of bloody mucus, exactly resembling the first. On the following day, there was much loss of strength: the animal lay upon its side, or staggered as it walked. There was great and insatiable thirst, with a small feverish pulse. But the most remarkable symptom was the occurrence, at intervals, of palpitation of the heart, accompanied by extraordinary force and sound, resembling that produced by long continued hypertrophy of that organ, in consequence of aneurism[1] of one of the large arteries. On the third and

[1] I once observed a similar condition in a young woman in St. George's Hospital. Each pulsation of the heart could be heard with great distinctness at a distance of two or three yards from the patient; during the paroxysms, there was the greatest difficulty of breathing; the countenance became anxious and livid, and a distinct thrill was communicated to all the arteries. After death, the countenance and upper part of the body were found livid from venous congestion. The lungs did not

fourth days, the animal was better, but there were still great thirst, fever, and occasional rejection of fluids from the stomach. On the fifth day, the symptoms became aggravated; there was extreme weakness, a tottering gait, excessive thirst, the eyes red and filled with gum; the nostrils were stuffed, swollen, and obstructed with mucus; and the lining membrane of the mouth was tumid, and of a violet red colour. In the middle of the day, there was a liquid greyish white evacuation, resembling pus in its odour, consistence, and appearance, mixed with some clots of putrified blood. Death occurred during the following night.

Post-mortem appearances. The mucous membrane of the eyes, nose, and mouth, was red or violet, and covered by a very abundant thick mucus. The lungs were of a dark colour, with some black patches, but still crepitant. The left ventricle of the heart presented several brown stains, resembling ecchymoses, which penetrated into its tissue. Its internal surface was of the colour of lees of wine, offering a singular contrast to that of the right side, which, however, contained a hard fibrinous concretion, two drachms and a half in weight, of a light yellow colour, resembling grease. This was of the same consistence throughout, everywhere free, with the exception of a portion of the size of a finger nail, which adhered to an irregular and apparently inflamed spot on the inner surface of the ventricle; no appearance of the injected fluid could be recognized in this clot, which was continued of the same colour and consistence into the pulmonary artery, and into the vena cava, the azygos, the axillary, and even the right jugular vein.

The intestinal mucous membrane, especially in the rectum, the duodenum, and a small portion of the small intestines, was of a violet red colour, inflamed principally in longitudinal stripes and in patches, which gave a mottled appearance, even to the outer surface of the intestines, before they were opened. This discolouration was not accompanied by any thickening of the tissues, nor by ulceration, and appeared rather the result of ecchymosis or hæmorrhage. The lining membrane of the rectum was principally affected, and its mucous glands were swollen and very prominent. This intestine contained puriform fluid, resembling the matter evacuated before death. The other intestines contained a very thick greyish white mucus. The mesenteric glands were inflamed, and appeared as if infiltrated with blood. The gall bladder was mottled on its surface by brown and violet patches, and contained black, thick, ropy bile, resembling melted tar.

XVI. *Effects of the introduction of Mercury into an artery.* An ounce and a half of mercury, mixed with water, was injected into the left carotid artery of a sheep. The animal immediately evinced pain, and stood immoveable upon its feet. The head was held down, there was stupor and heaviness, and the eyes were protruded and widely open. The fore legs subsequently became bent, and the head inclined over the right shoulder with a kind of convulsive rigidity, which continued

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readily collapse when the chest was opened. The auricles of the heart were greatly distended with black blood; the inner surface of the left ventricle presented a white patch, of about two square inches in extent; the lining membrane of the aorta, for several inches, was of a bright red colour; this extended round one-third only of the circumference of the vessel. No other structural change could be found in the heart or vessels, which would account for the symptoms observed during life.

till death. Two hours afterwards, the animal became comatose, with some convulsive motions of the limbs, and the left eye became red and inflamed. Death took place fifty hours after the operation.

Post-mortem appearances. The left eye was found in a state of suppuration, and contained mercury. Many of the branches of the left carotid also contained some mercury, which had not penetrated to the capillary system. All the organs supplied with these vessels were red, swollen, and inflamed, in consequence of the presence of the foreign matter. The thyroid gland, the tongue, the cheeks, and the lips, were, however, only affected as far as the median line, leaving the opposite halves pale and in their natural condition.

XVII. A drachm and a half of mercury, mixed with some warm water, was injected into the crural artery of a large dog. The animal evinced no pain, and walked resting slightly on the affected limb, which became sensibly colder. After the expiration of an hour, the animal refused its food, became restless, and indicated severe pain in the limb, which was now very hot. On the following day, the leg was swollen and œdematous. On the third day, there was extreme thirst, increased œdema, and great suffering. The animal was killed sixty hours after the operation.

Post-mortem appearances. No disease was found in any organ, excepting the affected limb. This was swollen and œdematous in every part; abscesses of different sizes had formed, which contained sanious fluid, mercury, and pus; some parts were in an incipient state of mortification, and gave out a considerable quantity of air. Globules of mercury were found in different parts, occupying usually the centre of the abscesses, and ran out upon the scalpel when incisions were made into the limb.

XVIII. *Effect of the injection of Oil into an artery.* Three drachms of olive oil were thrown into the crural artery of a large dog. Slight pain was experienced, and the limb became evidently cold, and the pulse under the tendo-Achillis could no longer be felt. Two hours afterwards, a like quantity of oil was again injected. The leg now began to inflame, and became tender. The following day, the whole limb was œdematous, much swollen, and very painful. Twenty-nine hours after the first experiment, the muscles of the thigh and leg, as well as the cellular tissue, were found in some places gorged with blood, and inflamed in lived patches; in others, infiltrated with yellow serum and gelatinous exudations. No oil could be detected in the affected parts.

XIX. An ounce of putrid water, in which some beef had been macerated, was injected into the crural artery of a middling-sized dog. The artery having been tied, the pulse ceased below the tendo-Achillis; the limb, however, preserved its usual degree of heat, offering a contrast in this respect to the last experiment. A considerable degree of fever and restlessness followed the operation; this continued the whole day and the following night, without any vomiting or evacuations, which so constantly followed similar operations upon the veins. The next day the limb was very painful, but not swollen; there was thirst, with the ordinary secretion of fæces and urine. On the third day, the animal was evidently better; the appetite had become almost natural, and he could walk more easily, although the limb was still very painful. In the night, there were some soft, almost liquid, evacuations. The fourth day, the

animal was evidently recovering, when an ounce and a half of very fetid and very concentrated fluid (derived from the maceration of beef), was injected into the crural artery of the opposite limb. The animal immediately evinced pain, accompanied by very violent and remarkable palpitation of the heart. It walked lame, keeping the leg raised, and soon became feverish and uneasy. The symptoms were exactly the same as after the first experiment. The leg became gradually more and more painful, extremely sensitive, but not infiltrated with serum. During the night, there was much expression of pain; the animal was in continual motion. Death occurred nineteen hours after the second injection. The limb had become swollen only within five or six hours previous to death.

Post-mortem appearances. The limb presented a very large quantity of bloody fluid infiltrated in all the tissues. The superficial muscles were black, and presented more or less the appearances of gangrene. The deep muscles existed as such no longer, but were entirely disorganized, and converted into a putrid pulp, resembling masses of the red lees of wine, extremely fetid, and disengaging a quantity of gas. The limb first injected was still swollen, and presented, in the interior of the adductor muscles, two or three cavities filled with a putrid bloody serum. In the chest, the lungs were healthy, as were also the right cavities of the heart; but the left cavities presented several reddish-black spots, scattered over the external surface. In the left auricle was a firm yellowish-white coagulum, adhering to an inflamed spot on its inner surface. The intestinal canal was filled with a brownish red fluid, resembling altered blood, which, in the stomach and duodenum, was of the colour of soot. The mucous membrane of these organs, as well as of the jejunum and rectum, were gorged with blood, of the colour of the lees of red wine, but without any inflammatory thickening of their coats.

XX. *Effect of the introduction of Air into an artery.* Seven or eight cubic inches of common air were injected gradually into the crural artery of a large dog. A peculiar rustling noise, depending upon the admixture of the air with the blood, accompanied the operation. No particular symptoms followed; but after some minutes the corresponding vein became distended with frothy blood, which moved with difficulty, and became stagnant in the vessel. The whole limb crepitated upon pressure, but no untoward symptom presented itself for more than half an hour. An ounce of water, to which seventy drops of medicinal prussic acid had been added, was now injected into the same artery. This produced no apparent effect upon the constitution.

A quarter of an hour after, an ounce of saturated solution of nux vomica was injected into the same vessel. This also was followed by no particular symptoms. An hour after the first injection, half an ounce of a weak infusion of tobacco was introduced into the same artery. Excessive pain immediately followed, accompanied by great rapidity of breathing. The animal now appeared as if he were going to die; however, he slowly recovered, appeared giddy and inclined to vomit, and kept himself in a continual state of restlessness. This condition was succeeded by fever, accompanied by extreme sensibility of the limb, and irregularity of the pulse. At the expiration of some hours, he appeared better; the pulse became more regular and less feverish, but the leg continued swollen, and extremely painful upon

pressure. During the night, the pain returned, indicated by howling and restlessness. There were several evacuations of fæces and urine. The following morning, there was great prostration with much fever, and continued howling. The limb was slightly emphysematous, greatly swollen, inflamed, and infiltrated with serum.

On the following day, two ounces and a half of water, in which some nux vomica had been boiled, were injected into the crural artery of the opposite limb. The dog expressed no pain; but, at the expiration of ten or twelve minutes, slight convulsive motions became evident, which were gradually converted into violent tetanic spasms. The animal threw himself backwards with his limbs extended, and died, after repeated convulsive attacks, an hour and a half after the last injection.

Post-mortem appearances. On opening the body, *no unusual appearances were observed in the limb* upon which the last experiment had been tried, but the opposite one was tumid and emphysematous, infiltrated with a greyish red frothy serum of a fetid odour. *The small vessels were obstructed by firm clots of blood.* The gall-bladder was greatly distended; and the intestinal canal contained a quantity of yellowish mucus.

(To be continued.)

ON PROCREATIVE POWER.

By C. H. F. ROUTH, M.D.Lond., Physician to the St. Pancras Royal General Dispensary.

The physiological and mathematical determination of the Procreative Power of Man is a portion of vital statistics, which, I believe, has not hitherto been properly investigated. When we contemplate the very interesting nature of the inquiry, and its importance as applied to individuals and national exigencies, and to emigration societies in particular, the cause of this omission is not very clear. Probably this neglect has, in a great measure, arisen from a natural disinclination in most persons to contend with controverted facts and dismembered statistics, and to perform the heavy arithmetical calculations required. There is, it is true, always more or less difficulty in investigating any new subject; and more especially in applying statistical data, originally drawn out for a totally different object. Moreover, many of the investigations are often, after all, founded on insufficient data; and what is still more embarrassing, frequently this disadvantage is not discovered till a series of heavy calculations, founded upon them, has been made, and a palpably absurd result has been brought out. Herein, therefore, is a source of great delay and annoyance. Further, when a question like the present involves a point in the physiology of man, he who would investigate its real nature, must consult statistical data, not only founded upon the experience of one, but of several nations. In making, however, this comparison, he is met at once by a most embarrassing difficulty. The statistics of no two nations involving questions of population are founded upon the same general plan; and it thus often happens, that a point elicited by the examinations of the vital statistics

of our own country, cannot be confirmed by those of another. Even among ourselves, the plan adopted by the Census of Ireland Commissioners is totally different from that adopted by the Census Commissioners of Great Britain. The arrangement of years is different; and several most important omissions are made. Thus, in the returns of Great Britain, no table of the number of married, unmarried, and widowed persons is given, in which the ages are at the same time distinguished; nor is there any table of fecundity; perhaps, also, conclusions are sometimes made, the truth of which may be questioned. In the following pages, we shall often have to recur to these difficulties, by which, unfortunately, our conclusions are in some degree vitiated. Indeed, so frequently have I been embarrassed in the course of this inquiry, by such obstacles, that I have been more than once induced to abandon it altogether. Assisted, however, by an excellent mathematician, and by the prospect of probably calling attention to a subject of general utility, of disproving, perhaps, a few of the dogmas of the present day on the exigencies of an overcrowded population, and pointing out some foundation upon which something like a scientific superstructure might be raised, I have persevered in my endeavour to accomplish my object. Imperfect, nay, very imperfect, as the attempt must necesarily be in many of its details, it may, nevertheless, not be altogether presumptuous to hope, that the perusal of the succeeding pages may, by calling the attention of the learned to this portion of vital statistics, lead to the collection of such data as may hereafter place this branch of science upon something like a correct foundation.

VARIETIES OF PROCREATIVE POWER.

Procreative Power is that power inherent in every animal, which, when brought into practical operation by the union of the sexes, assures the continuance of the species. Man, in his animal relations, possesses this power in common with all created beings; yet, by the very fact of his being an intelligent creature, his procreative power must be regulated by, and depend upon, several auxiliary influences, all of which must modify or bear upon its general action and exercise in the inhabited globe. Of these, three only will be considered; the others may be incidentally alluded to as we proceed, but they affect the question so little, that they need not be particularly enumerated.

I. A person must have obtained that physical constitution of body which enables him to procreate; in other words, he must have reached the age of puberty. Thus, children generally cannot have this power. Having once obtained the age of puberty, a person will continue to have this power for a number of years; at the end of which time, a new change will come upon the animal organism, and the power of procreation will be lost; in other words, the procreative power has its limit as well as beginning. But between the period of its commencement and termination it is not equally powerful; there is a particular age in which it is most powerful, most effective; in other words, a maximum age of procreative power. For instance,—it is not sufficient to know that a man is within the limits of procreative ages, but the actual age must be known also, as according to that age he will have more or less

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procreative power. This procreative power we have denominated as the *Physiological Procreative Power*. Furthermore, it may be convenient to distinguish it as of two kinds. (*a*) A man of a certain age will have a certain average amount of physiological power when brought in union with *women of all ages*. It will be greatest, it is true, when the wife is at her maximum age of procreative power, and least when she is reaching the limits of its disappearance: but, nevertheless, taking women of all ages, the average physiological procreative power will be expressed by a fixed number. (*b*) For every different age of the female, with the same age as the male, it will be different, and expressed by a different number. Hence the necessary division into two kinds,—1st., the *average* physiological power, and 2nd, the *united* physiological power. Both of these will necessarily have a certain and fixed relation one to another.

II. It will be likewise affected by the social condition of the person, especially if he be married or not married. The procreative agents of a nation are, if not exclusively, at least eminently, the married persons. Taking our population of this year (1850) as calculated by the Registrar General, it amounts to 29,679,000, with an annual increase of 305,000; of these, 285,000 are exclusively produced by married persons, and yet the difference in numbers between the married and unmarried population is expressed by the numbers, 100 and 293, or nearly 1 to 3. If out of this number we separate the active procreative agents, or at least persons possessing physiological procreative power, from children and old persons who are incapable of procreation, the numbers will be in the proportion of about 260 unmarried or widowed to 100 married. It follows, that about 9,000,000 persons only, or about 4,500,000 of couples out of the 29,000,000, produce, annually, this increase. We have said that the increase is not exclusively produced by married couples; because, although with most unmarried persons the procreative power is dormant, still with some it is active, as in the production of illegitimate children. Thus the increase to the population from this source, will be 20,000 out of the 305,000. This kind of power, dependent upon the social condition of a person, we have contra-distinguished as *Practical Procreative Power*, which may be further subdivided, according as the person is married or unmarried, into *connubial* or *ex-connubial* practical procreative power.

III. Procreative power will also be modified in the unmarried by two other contingent circumstances;—1st, the chance he has of getting married, or becoming possessed of connubial procreative power;—2nd. it depends also on his expectancy of life. Thus, supposing two persons marry at the same time, it is clear that the one who runs the risk of dying soonest, *cæteris paribus*, cannot have the prospect of having as many children as the other. This kind of procreative power we have termed the *Expectant Procreative Power*. Children, in the two last cases, may have their power represented by a certain number, just according as a child has a chance of reaching a marriageable or procreative age; and thus of becoming a practical procreative agent. In this manner, they may be looked upon as having a nominal or prospective amount of procreative power.

To recapitulate: every person's procreative power is made up of—1st, his physiological, 2nd, his practical, and 3rd, his expectant procreative power. This totality may be expressed by the term *Absolute Procreative Power*, which may again be subdivided into two kinds, according as the individual is unmarried or married,—the *Ex-connubial* and *Connubial* Absolute Procreative Powers.

PHYSIOLOGICAL PROCREATIVE POWER.

In fixing the limits of Procreative Power, it should be remembered, (more especially for the practical bearing of the question), that it is not so much the early limit we should insist upon and endeavour to fix accurately, as the late. And this is manifest, because most persons at these later ages are married, and therefore in the position of being active procreative agents. In fixing the limit of the disappearance of procreative power too low, we are therefore materially vitiating the result. In the earlier ages it is not so important, as whatever be the exact age at which the power begins, it is seldom active before sixteen, and the proportion of increase of population by persons of this age is comparatively unimportant.

The first thing to be remarked in reference to these limits, is that procreative power will differ according to the sex. Its limits have not been hitherto very distinctly made; and yet there appear to be four ways in which it may be estimated in the female, and three in the male. In the former only, by the first appearance and cessation of the catamenia; and in both sexes,—1st, by the ages at which crimes arising from sexual desires are committed; 2nd, by the fecundity tables, where the age of the parents is given at the birth of their children; and 3rd, though here less decidedly, because fallacious influences more readily vitiate the result, by the ages at which persons marry.

I. *(a)* In the female, the catamenia are usually said, in this country, to begin at fifteen, and to disappear at forty-five. In warm countries, however, they are believed to begin and end earlier. Upon the latter point, however, and in the absence of any statistical data upon which alone any dependence can be laid, I am inclined to be very sceptical. Looking to the limited opportunities for observation of travellers, upon whose statement this opinion is founded, and other causes of error, as noticed by Dr. Churchill, I am inclined to believe that the instances recorded by them are exceptions, precisely similar to those cases recurring amongst ourselves; and that there is, probably, no such great difference in the age of uterine activity in different countries. The annexed table, collected from various sources, will shew how little the opinion before given, touching the earlier appearance of the catamenia in warm countries, is to be depended on, as there appears to be, in reality, very little difference between its commencement in Marseilles, England, and Germany. In the following table, consisting of 4433 cases, in which the first appearance of the catamenia was noted, 4 cases of M. Brierre de Boismont, occurring, respectively, one at 5 years old, one at 7, and two at 8 years old, have been omitted, as they appear to have been rather cases of *lusus naturæ*, and as their insertion would only affect the result in the second place of decimals.

[TABLE,

Table, showing the Age at which the Catamenia occurred in 4,433 Females for the first time.

Age.	Marseilles and Toulon.	Paris. M. Despines.	Gottingen. Osiander.	Lyons. Petrequin and Bouchaincourt.	Brierre de Boismont.	London. Dr Murphy.	Manchester. Mr. Roberton.	London. Dr. Lee.	Dr. Henry Bennet.	London. Dr Routh.	Total.	
9	0	1	0	0	10	3	0	11	0	0	25	
10	0	0	0	5	29	0	0	55	5	0	94	
11	6	3	0	14	93	15	10	52	16	2	211	
12	10	14	3	26	105	37	19	86	28	8	336	
13	13	6	8	47	132	57	53	153	35	10	514	
14	9	18	21	50	194	107	85	204	42	19	749	
15	16	14	32	76	190	119	97	201	42	16	803	
16	8	7	24	79	141	110	76	154	26	15	640	
17	4	6	11	58	127	57	57	101	20	18	459	
18	2	5	18	38	90	34	26	78	23	2	316	
19	0	8	10	21	35	9	23	33	10	1	150	
20	0	0	0	9	30	6	4	23	6	2	80	
21	0	3	8	5	8	3	0	6	0	0	33	
22	0	0	0	0	8	2	0	2	0	0	12	
23	0	0	2	1	4	0	0	1	0	0	8	
24	0	0	0	3	0	0	0	0	0	0	3	
Total	68	85	137	432	1196	559	450	1160	253	93	4433	Mean Age, 14·91

Touching the disappearance of this function, we have but few statistical data, for which reason, much reliance cannot be placed in the result obtained. There is also in the following tables a great source of fallacy, especially applicable to those cases where the catamenia did not disappear till a very late age; viz., the possibility of uterine diseases existing, an affection by no means rare in old females. It is, therefore, here subjoined, more for reference than as any actual ground of conclusion.

Table, showing the Age at Cessation of the Catamenia.

Age.	Mr. Roberton.	Dr. Routh.	Total.	Age.	Mr. Roberton.	Dr. Routh.	Total.	
35	1	0	1	50	26	1	27	
38	0	2	2	51	2	1	3	
40	4	1	5	52	7	3	10	
42	2	0	2	53	2	0	2	
43	1	0	1	54	2	0	2	
44	3	3	6	55	0	1	1	
45	4	5	9	57	1	0	1	
46	0	1	1	60	2	0	2	
47	3	3	6	70	1	0	1	
48	10	3	13					
49	7	0	7	Total	78	24	102	Mean Age, 48·49

The fact, moreover, should not be lost sight of (as bearing on the relation between the cessation of menstruation and the cessation of procreative power), that it is not uncommon for females to become pregnant, in whom no menstrual period has occurred between the present and a

former pregnancy. This, I believe, is a point admitted by all accoucheurs. Indeed I have known a few cases, in which this absence of menstruation has persisted through several consecutive pregnancies; and, lastly, although we cannot reduce this statement to numbers, I believe that these examples most frequently occur at the later ages of procreation. In such cases, therefore, the cessation of the catamenia is no index of the cessation of procreative power.

(*b*) From the Fecundity Tables a more certain result is to be obtained, more especially with reference to the cessation of this physiological power. First, from the Irish Fecundity Table, where the ages of the parents on the wedding-day, as well as the number of children born during a period varying from 1 to 11 years, are given.

Table of the Number of Marriages, and the Issue from such marriages occurring in the Eleven Years ending 1841, *for women of the ages under* 17, 46-55, *and above* 55.

Age of Husband.	Age of Wife.	No. of Marriages.	Prop. of Children to 100 marriages	Age of Wife.	No. of Marriages.	Prop. of Child. to 100 Marrgs.	Age of Wife.	No. of Marriages.	Prop. to 100 Marriages.
Under 17	Under 17.	661	256	46 to 55	1	0	Above 55	1	0
17—25		9,847	262		35	51		3	0
26—35		4,066	252		145	51		12	0
36—45		313	248		227	39		15	20
46—55		36	205		428	22		52	10
Above 55		18	128		295	10		136	12
Total...		14,947	257		1,131	26		219	12

In the two following tables, we have the proportion of pregnancies of 2509 females, admitted as lying-in patients in the Westminster General Dispensary, under Dr. Bland. There were 36, or 1 in 58, admitted pregnant between the ages of 15 and 19. Thus:

1	between	15	and	16
1	... ...	16	...	17
2	... ...	17	...	18
10	... ...	18	...	19
21	... ...	19	...	20
36				

In all there were—

85	between	15	and	20	or	1	in	25
578	... ...	21	...	25	...	5	...	19
699	... ...	26	...	30	...	1	...	3
407	... ...	31	...	35	...	1	...	5
291	... ...	36	...	40	...	3	...	22
336	... ...	41	...	45	...	1	...	58
6	... ...	46	...	49	...	1	...	350
2509								

The following is a list of 1,527 cases admitted in the St. Pancras Royal General Dispensary, in the practice of Mr. Smith, Dr. Cox, and Mr. Greenhalgh.

Age.	Number of Conceptions.	Age.	Number of Deliveries.	Age.	Number of Deliveries.	Age.	Number of Deliveries.	
16	1	26	83	36	62	46	2	Mean Age, 30·12
17	0	27	80	37	47	47	2	
18	5	28	85	38	48	48	2	
19	16	29	96	39	31	50	2	
20	33	30	139	40	50	52	1	
21	56	31	61	41	7	55	2	
22	56	32	88	42	17	57	1	
23	67	33	59	43	15			
24	77	34	71	44	9			
25	83	35	69	45	4	Total	1,527	

The Fecundity Table for Sweden and Finland, gives about the same limits.

Age.	Annual Average Number of Deliveries.	Proportion to 100 Females living.
15 to 20	3,298	2·48
20 — 25	16,507	12·56
25 — 30	26,329	21·64
30 — 35	25,618	22·82
35 — 40	18,093	18·32
40 — 45	8,518	9·54
45 — 50	1,694	2·28
50 — 55	39	·05

Thus, as far as we can judge from these fecundity tables, it does not appear that women younger than fifteen have borne children. In the St. Pancras Dispenary table, there are three cases only, where females fifty-five and above fifty years old were delivered. In the Irish table, however, a greater number of such cases occur. Thus far it is proved, that it is inaccurate to say too generally, that women cease to procreate at forty-five. It appears that, in ten years in Ireland, out of 427,977 marriages of women of all ages, there were 1,350 marriages of females between forty-six and fifty-five, and above fifty-five, with a mean of fourteen children to 100 marriages.

(*c*) As I have before stated, the marriage tables afford some degree of information, with regard to the first appearance of procreative power. It is natural to suppose, that in an enlightened age, like the present, parents would prevent marriages of their children if the signs of puberty were absent; and upon this presumption, we might infer the existence of procreative power in all couples marrying early. The marriage tables, however, being for the most part arranged by ternal, quinquennial, and decennial, and not by annual periods, the precise age at which marriage occurred is not given. In the Irish tables, all early marriages are included in the term under seventeen, which may, and most likely does, only include two years: fifteen and sixteen. With females in Ireland, the number of such marriages is stated to have been, in the eleven years preceding 1841, 14,947. The English marriage table includes the marriages under the general terms, between fifteen and under twenty; but, excepting that the mention of the former age justifies the belief that some marriages do take place at fifteen, the division is too

ambiguous to enable us to say distinctly, what is the earliest age at which they occur.

(*b*) The Criminal Tables give no very good index as to the early period for females. In the three following, that for England and Wales is defective, as it unites the sexes.

Table of Sexual Offences for the United Kingdom.

	England and Wales. 5 years.	Ireland. 1845-7.		Scotland. 1842-1846.		Ireland and Scotland	
	Centesimal proportion of offenders.	To 100,000 population.					
	Persons.	Males.	Females.	Males.	Females.	Males.	Females.
Under 12	—	—	—	—	—	—	—
12 — 16	3.4	1·46	—	4·14	—	5·60	—
16 — 20	18·0	17·73	3·81	16·19	4·62	33·92	8·43
21 — 30	32·8	31·24	7·02	17·80	7·94	49·04	14·96
31 — 40	22·4	16·04	3·26	13·19	4·70	29·23	7·96
41 — 50	12·8	7·16	·29	5.97	·28	13·13	·57
51 — 60	6·9	4·18	·76	4·40	.02	8·58	·78
Above 60	3·7	3·03	—	4·54	—	7·57	—

In the above tables, in the statistics for Ireland and Scotland, the offences only, which may be said to arise from sexual desire, are included. These, in addition to two of an unnatural kind, which need not be specified, are rape and carnal abuses of girls; and, in addition, bigamy, concealment of the birth of infants, and child-dropping. Bigamy, because the motives that induce a man to commit it, are the same as those which induce him to marry; and the two latter, because they invariably occur among persons giving rise to illegitimate children, whose sexual passion may be, therefore, said to have led them into error. It is true, that other motives may sometimes have their influence, but these cannot be now considered. The same objection to defining the age of the first commencement of the sexual desire, offers in this as in the case of marriages. Concerning the limit, however, in females, sexual desire does not appear to exist above sixty. Duly weighing, therefore, all these results, it appears most correct to limit the existence of procreative power in females, between the ages of fifteen and fifty-five.

II. (*a*) In the case of males, our evidence is of a different kind. In one respect, it cannot be so precise. We labour under the disadvantage of not having a function like that of the catamenia, upon which to found our conclusions, It is usually said, that the approach of puberty in males is marked by "the change of voice, growth of hair on the pubes, and the occurrence and discharge of semen." We are not, however, acquainted with any statistical data in which the age of the first appearance of these signs, either single or conjointly, has been noted. The only tables which appear to give some positive information on this subject, are the Irish Fecundity Tables. Here again, however, we have the two undefined terms of "under seventeen and above fifty-five" occurring, the former of which may include two or three, the latter several years. It is useful, however, as pointing out, approximately, the periods at which procreative power is effective.

Table of the number of marriages, and the issue from such marriages occurring in the eleven years, ending January 1841, *for the ages of husbands under* 17, 46 *to* 55, *and above* 55.

Age of Wife.	Age of Husband.	No. of Marriages.	No. of Child. to 100 Marrgs.	Age of Husband.	No. of Marriages.	No. of Child. to 100. Marrgs.	Age of Husband.	No. of Marriages.	No. of Child. to 100 Marrgs.
Under17	Under 17.	661	256		36	205		18	128
17—25		757	256	46	1,516	210		240	169
26—35		92	203		2,469	167	Above	461	119
36—45		4	—	to	1,335	70		429	43
46—55		1	—		428	22	55.	295	10
Above55		1	200	55	52	10		137	12

It will be seen by this table, that the number of marriages of males at the earlier ages is less than that of females, still they do occur. In Ireland for instance, out of 428,802 marriages, there were 1516 where the age of the husband was under seventeen.

(*b*) The English Table for Marriages is still less precise. The early marriages of males are included under the given nominal period from fifteen to twenty, and only seem to justify the inference, that, while at this early period a few take place, their number is small, as compared with females; since, out of 20,437 marriages given, only 537 among the males, and 2711 among the females, occurred, where the parties were respectively between fifteen and twenty years old. As a rule, however, it is believed, that in England, men seldom become active procreative agents before sixteen; and this number appears to receive some confirmation from the marriage tables.

(*c*) The Criminal Tables before given, give the cypher 5·60, as the amount of sexual offences committed by males between the ages twelve and sixteen, to 100,000 of the population of every age. This number, however, compared with the next period, sixteen and twenty-one, is very insignificant; but in either case, the terms are too vague, and the period too lengthened, to enable us to infer, very accurately, the early age. Moreover, the offences that do occur at these ages, twelve and sixteen, are precisely those where other motives than sexual desires may influence the perpetrators. Duly weighing all these results, it seems, therefore, more correct to fix the age of sixteen as the early limit. But if puberty appears *later* in males than in females, the physiological procreative power certainly *persists longer*. Thus, in the marriage ages from forty-six to fifty-five, their power, in 1 to 11 years after, is represented by the number 144, while that of females, for the same period, is only 26; and for the ages above fifty-five, it is represented by the number 77, that for females for the same period, being only 12. Indeed, a male, from forty-six to fifty-five years old, has almost as much procreative power as a female from thirty-six to forty-five, whose power is represented by the numbers, 1 to 99. It is clear, however, from the table given above, that it persists above fifty-five, but it does not shew how long it continues after this period.

The Criminal Tables will, perhaps, justify a further inference. The sexual offences committed in Scotland and in Ireland, for the period

above sixty, (i.e., from sixty to sixty-six) are represented by the cypher, 7·57, a number almost equal to that of females for the ages thirty-one to forty, and exceeding that for males for the ages sixteen to twenty-one. So far, then, as these tables go, they seem to justify the conclusion, that men have procreative power above sixty.

M. Bernard Hebellen, in a paper published in the *London Statistical Society's Reports*, speaks of sixty as the age in males, from the probable marriage of whom, no issue could be expected. Our own experience, we must say, is not confirmatory of this opinion, as we have known several instances where, with a young wife, men, as late as sixty-eight, have had several children. Here, however, we speak with great diffidence, from the absence of statistical data by which to support our own opinion. The Marriage Tables will, in some measure, however, assist us in fixing this limit. Thus, if we take the number of marriages occurring to 100 of the unmarried population, living between the ages of fifty-five and eighty-five, as given, for instance, in the Irish tables, and take the mean age, we obtain sixty-six as the average age. But here, another objection presents itself, as regards this limit deduced from the Marriage Tables. When persons marry late, they do so, frequently, from motives of convenience, apart from any sexual desire. According to the number of these marriages of convenience, there will necessarily be a tendency to diminish the value of marriages, as a test of sexual desire. In this calculation we labour, therefore, under the disadvantage of including all these exceptions. Yet it is convenient (as it is the end of a quinquennial period), to fix, for practical purposes, the age sixty-six or sixty-five, the former of which is the mean of all marriages occurring after fifty-five, as the limit of the duration of physiological power. It is true, we may thus give rise to more or less error for the last three or four years; but this is inconsiderable, as it affects only the results for the period above fifty-five, and these only in the second place of decimals.

AVERAGE PHYSIOLOGICAL PROCREATIVE POWER.

In fixing the limits of Procreative Power, it also appears, *à priori*, that the maximum age of fecundity will be that at which most marriages occur, and in which those offences, arising from the desire to gratify sexual passion, are most numerous. How far these suppositions are correct, the following inquiry will, in a great measure, shew.

In relation to the first of these points, it is laid down by Captain T. A. Larcom (*Journal of Statistical Society*, vol. viii, p. 212), that the most marrying race shew the lowest cypher of fecundity; and the authority of Mr. Gregg, in his work on the *Social Statistics of the Netherlands*, is adduced as evidence of this fact. It is not purposed to enter upon this controversy at present. It is presumed, however, that it will not be denied to be rational, *à priori*, to suppose, that with any person whose moral and better feelings are not entirely extinct, the marriage tendency will be greatest when his sexual passions are strongest. The number of marriages, *at all ages*, may not bear a direct proportion to sexual passion, for the reasons before given; but the maxima in both cases will agree.

If, now, sexual passion be looked upon as an index of fecundity, or, in

other words, the most impassioned be allowed to have most procreative strength, then marriage tables, tables of sexual offences, and of fecundity will, in so far as the maxima are concerned, pretty nearly agree. Now, so far as analogy among the brute creation goes, it is favourable to the supposition. An animal, when in heat, is in the condition most favourable to procreation. Indeed, in most, if not in all cases, this preliminary excitement is a *sine quâ non* to conception. Woman, it is true, differs, in this respect, from other animals, in being always capable of procreation, so long as the catamenial function persists; but we are not aware of any data that shew, that when, within procreative ages, the sexual passion is strongest, she is not then most liable to conception. We have already spoken of morbid feelings which may exist without procreative power; but, in default of statistical data to prove this assertion, it behoves us to inquire how far these opinions are carried out by the indirect statistical documents at our command.

(*a*) At what age do most people marry? Tables are given for several countries, in which the number of marriages that take place annually, at different ages, is set forth. It is right, however, in deducing our conclusions, also to take their ratio to the population of unmarried persons living at every age. Thus, there may be more persons living at one age than at another. In the former age, therefore, more marriages may positively take place, though relatively fewer than in the latter age. Moreover, in making this comparison, the married should be excluded from the population, as these can give no index as to the desire of marriage among the unmarried.[1]

[1] This disadvantage exists in a marked manner in our English tables. We have no table of the unmarried population for corresponding periods to make the comparison; therefore, by including the married, our conclusions are necessarily vitiated. The calculation has, however, been made, and the table is given here for reference, to make this source of fallacy more evident by comparison with other tables.

MALES.				FEMALES.			
Age.	No. of Weddings.	Weddings to 10000 population.	Reduced to a maximum of 100	Age.	No. of Weddings.	Weddings to 10000 population.	Reduced to a maximum of 100
15—20	537	5·94*	3·70	15—20	2,711	27·40*	23·79
20—25	10,383	12·58	7·78	20—25	10,424	107·06	92·97
25—30	5,103	74·35	46·42	25—30	3,951	50·03	43.44
30—35	1,900	29·35	18·32	30—35	1,498	21·15	18·36
35—40	944	18·97	11·84	35—40	739	13·95	12·11
40—45	603	12·00	7·78	40—45	532	9·94	8·63
45—50	371	10·59	6·61	45—50	273	7·18	6·23
50—55	271	7·68	4·79	50—55	161	4·17	3·62
55—60	147	6·71	4·19	55—60	69	2·04	1·77
60—65	112	4·60	2·87	60—65	55	1·99	1·72
65—70	41	2·96	1·84	65—70	17	1·05	·91
70—75	15	1·24	·76	70—75	5	·35	·30
75—80	6	·91	·56	75—80	2	·26	·21
80—85	4	1.19	·67	80—85	...	...	...

* Maximum ordinate for Males, 160·15 / Females, 115·15

From these results, it appears that most women marry at the age of 22·80, and most men at that of 27·80, or persons in general, at 24·94. In the American table the same error exists, and males and females are not distinguished. The maximum age, however, obtained, differs but little from that of the English table, namely, 25·84.

In the Irish table, in which the number of married and unmarried persons, for corresponding periods, are given for comparison, the result obtained is much higher; the maximum age being, for males, 31·84, and for females, 26·34, or for persons, 28·61. The total number of marriages that took place in the 11 years, from 1830 to 1840, inclusive, are given in the following table; the calculation is made upon the population of 1841. It would have been more correct to use the mean population for all these 11 years; but in the numbers now obtained, positive accuracy is not so much sought after, as results for practical application.

Marriage Table for Ireland, 11 *years ending* 1841.

MALES.					FEMALES.			
Age.	No. of Weddings.	To 10000 population.	To a maximum 100.	Maxim. age, 31·84	No. of Weddings.	To 10000 population	To a maximum 100.	Maxim. age, 26·34
Under 17	1,516	80·81	·92		14,941	831·11	13·3	
17—25	191,677	3021·23	34·4		305,312	4977·90	79·26	
26—35	193,639	8335.63	94·8		98,195	4981·20	79·32	
36—45	33,830	4115·57	46·7		8,279	701·61	11·1	
46—55	5,836	1128·82	12·7		1,131	101·61	1·5	
Above 55	1,579	167·76	1·6		219	10·63	·16	

Other marriage tables were obtained; but as a comparison could not be made with the unmarried population only, and as, moreover, the population was frequently grouped in periods different from those in which the marriages were arranged, the deductions made were considered insufficient, and accordingly rejected.

(*b*) The Criminal Tables before given, bring out maxima, which, after all, are not very different from the above. That for England and Wales gives 28·50 as the maximum age for persons; the Irish marriage tables, taken also for persons, give 29·09: two results which come very near one to another.

The United Table of Sexual Offences committed in Ireland and Scotland, gives—

Maximum age for Males	28·00
Ditto for Females	26·66
For Ireland alone—for Males	27·01
Ditto for Females	26·55
For Scotland alone—for Males	28·09
Ditto for Females	26·77

These results, for females, may be said almost entirely to agree with those obtained from the Marriage Tables, though there is some slight difference as regards males. The reality is, perhaps, that for the ages between twenty-seven and thirty-one in males, there is very little difference in the amount of procreative power. The mean of the Marriage and Criminal Tables is 29.

There is another way, in which this apparent discrepancy may be explained. The calculations of the criminal tables are made upon the whole population, married and unmarried. This was purposely done, because bigamy is an offence belonging only to married persons, and to which, therefore, a comparison with the unmarried, exclusively, could not apply; but, it has the disadvantage of affecting, perhaps, the propor-

tion, in relation to the other sexual offences. These, (though, perhaps, occasionally, a married man may be implicated) are chiefly committed by unmarried persons. On the other hand, however, the amount of this error could not be shewn, as there is no table of the unmarried and married persons, for the periods of age here given; and the comparison was, therefore, impossible. Having thus to choose between two evils, we selected the least, though a simple consideration of the above facts would explain, why the criminal tables might give a lower maximum. The nature of the sexual offences committed by women, explains why their maximum cypher is not equally vitiated.

(*c*) We would naturally expect, however, that the accuracy would be much greater, if we deduced this maximum from Fecundity Tables. Undoubtedly, this would be the case, if these were constructed with more care; and the calculations would be as brief as correct. It would suffice to know the age of the parents at the birth of every child; and that age in which, relatively to the population of active procreative agents, most children were born, would be the age of greatest fecundity. Unfortunately, no such tables are given; and we are, therefore, compelled to make use of tables comparatively defective.

The Swedish table, before given, would be one of the most valuable in determining this age for females, if the number of active procreative agents were known, relatively to every age. In the absence of these data, it must be rejected. As, also, Dr. Metcalf's Table for Fecundity, in New York, is arranged in periods of years, for which no corresponding table of the active procreative agents (for similar periods), is given, it must be also set aside. Thus are we reduced, for females, to the Westminster and St. Pancras General Dispensary tables, and to the Irish tables. For the males, the Irish table is the only one upon which we can depend.

It may be well to mention here, in speaking of the Irish tables, one capital defect in them, which, if not insuperable, is at least calculated to give rise to very erroneous results. This table, given at page 487 of the Report of the Census Commissioners for Ireland, includes, in the total, not only the children born of parents, while only they continued within the ages in which they married, but also for a period, varying from 1 to 11 years after, when their fecundity would be the same as that of persons who married at a later period. To make our meaning more plain by an example: 437 females married while under seventeen, in the year 1830, and in 11 years they had 883 children. This number is retained, as measuring the fecundity of the age under seventeen; whereas, in reality, these females, in these eleven years, had passed through the phases and procreative power of the next period, *i.e.*, from seventeen to twenty-five, and necessarily enjoyed the procreative power of that period. The number, therefore, as measuring the fecundity to 100 marriages, for this period, should be divided, at least by 11. If the number of those who married in 1831, be taken, the measure of fecundity is at least 10 times too great for this period, and so on. The effect of this is to raise, unduly, the fecundity of the earlier age, up to the maximum, and to lower that of those below this maximum. In order to obviate this difficulty, we have been satisfied to deduce our table of Average Physiological Power from the two years, 1839 and 1840,

dividing by 2 the number of births in the former years. This, it is true, introduces a small amount of error; but it is very inconsiderable. The advantage thereby obtained, quite makes up for this trifling error, by enabling us to correct the inaccurate numbers obtained from the consideration of a smaller number of marriages and births (especially during the earliest and latest periods), occurring in the single year. We begin first with males.[1]

Table of Fecundity for Males for the years 1839 *and* 1840.

Age.	No. of Children to 600 Marriages.			To 100 married population.	To a maximum of 100.	
	1840.	1839.	Total.			
Under 17	60	117	177	16·42*	31·59	*Max. ordinate, 51·89
17 — 25	197	154	351	29·25	56·46	
26 — 35	157	160	317	26·42	51·00	
36 — 45	158	161	319	26·58	51·00	
46 — 55	92	110	202	16·83	32·49	
Above 55	60	94	159	13·25	25·57	

The maximum for each of these years separately, differing only by 78. That for 1840 being 25·40; that for 1839, 26·22; mean, 25·81.

There can be no doubt, that this maximum is too low; and the reason is obvious. Both the years 1839 and 1840 are defective for husbands under seventeen, as it happened that no marriages took place of males of this age with females from thirty-six and upwards, and only thirty-nine altogether with females under thirty-five. The cypher of fecundity obtained for this age of males is, therefore, much too high; and as it

[1] As before said, we have three tables from which we may deduce the average Procreative Power for females: the Westminster Dispensary, the St. Pancras Dispensary, and the Irish Fecundity Tables. To deduce from the two former the maximum of fecundity, it is necessary that we should have a table of the number of active procreative agents, *i. e.*, the married, as these form so much the greater part among these. There is no such table for England. If to remedy, however, this error, we assume that the proportion in England and Ireland is about the same, which, after all, may not be very far from the truth, we have the following results.

Age.	Number of Deliveries.					To 100 married population.	To a maximum of 100
	St. Pancras Dispens.	Westminst. Dispens.	Irish Fecundity Tbls.		Total.		
			1840.	1839.			
Under 17	1	2	86	175	264	24·78	8·8
17—25	393	661	11,190	12,319	23,563	228·50	82·0
26—35	831	1,106	3,980	4,898	10,815	45·98	52·4
36—45	290	698	160	245	1,396	64·66	23·2
46—55	11	42	5	5	63	2·91	1·04
Above 55	1	0	0	2	3	·21	·14

But in the above table, the number for the period between 26 and 35 is evidently incorrect. This arises from the accidental circumstance, that the number of deliveries in the Westminster Dispensary for this period is so great. The mean of 64·66 and 228·5 is about 146. If this number be substituted, we have for maximum age, 24·01, and maximum ordinate, 278·60. The two Dispensary Tables, worked separately, give results not very different from that above obtained, where the two are combined.

In applying, however, the proportionate numbers for married persons of all ages to the Dispensary Tables, as the usual number of children born to one marriage is

necessarily enters into the calculation for the maximum, the maximum age comes to be too low.

Such as it stands, however, the analysis of the above table brings out the following fact. The procreative power increases exceedingly quick at first, till it reaches its maximum in about eleven years. For the next ten years it diminishes gradually. For the next twenty it remains in *statu quo*, till in ten years more it comes to equal what it was at the age under seventeen, and then gradually, but slowly, disappears.

In estimating the average physiological power for females, we have also made use only of the Irish tables. The two dispensary tables have been rejected, as the correct proportion to the married population of the respective periods could not be made out. An attempt, however, has been made to rectify this disadvantage; and the results are appended for reference in a note.

The Irish Fecundity Tables, worked for two years only with the same precaution observed as in the case of males, fix the age somewhat higher.

Table of Fecundity for Females, for the years 1839 *and* 1840.

Age.	No. of deliveries to 600 married.			To 100 population.	To a maximum of 100.
	1840.	1839.	Total.		
Under 17	104	114	218	36·33	44·96
17—25	230	240	470	78·33	96·94
26—35	185	265	450	75·00	90·28
36—45	129	118	247	41·17	50·95
46—55	81	33	114	19·00	23·51
Above 55	0	37	37	6·16	7·06

giving as a maximum age 26·66, and maximum ordinate 80·80. The maximum age for 1840 alone, is 25·13.

This result would seem to fix the maximum of male and female about the same for average physiological power, and, if anything, rather higher

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estimated in England at three, the total number of deliveries should be trebled, to give the number of marriages.

Age.	Number of Deliveries.				West-minster.	St. Pancras.
	West-minster.	St. Pancras.	Westmins. to a maximum of 100	St. Pancras to a maximum of 100		
Under 17	2	1	65·32	53·76	Max. age.	Max. age,
17—25	661	393	72·22	70·55	22·29	23·67
26—35	1,106	831	55·93	52·92		
36—45	698	290	52·12	24·10	Max. ord.	Max. ord.
46—55	42	11	·34	0·15	73·60	75·48
Above 55	0	1	·0	·019		

It may be here remarked, that the mean maximum age of the two Dispensary Tables is 22·98, a result very near that of the maximum age of marriage among English females. It should, however, be borne in mind, that apart from the error of the English Marriage Table before mentioned, the results are not of equal weight with those of the Irish Tables. The English Marriage Table is calculated upon 40,000 marriages, and the fecundity only from 4,196 deliveries. The Irish are made from upwards of 400,000 marriages, and 17,000 deliveries.

for females than males. The single year 1840 alone makes it higher for males. This is manifestly an error, owing to the accident before noticed. Yet, as the deduction of average procreative power from fecundity tables may be useful for reference, it is perhaps convenient to consider the maxima in both males and females to be 26, at the same time, as by reducing to the same unit, the numbers deduced from the marriage tables, other numbers for the ages 21½, 31, and 41, may be annexed in another column for comparison, and this is what has been done in the annexed table for physiological procreative power.

That there is a curve representing the exact inclination of procreative power, cannot be doubted. Its exact determination, however, is impossible, when such distant intervals as ten years are given as ordinates between the ages. The circle would perhaps be the nearest curve that could be selected, but it very much increases the amount of arithmetical labour necessary. Fortunately, for practical purposes, such accuracy, however desirable, is not absolutely needed. If we suppose these several ordinates joined by a straight line, the procreative power of every age may in this manner be calculated, which, if not exactly correct, may yet be very useful in its application. Accordingly, in the following table, the procreative power for every age has been so calculated.

Table of Average Physiological Power, calculated from the Irish Tables.

Age.	Males.		Fem.	Age.	Males.		Fem	Age.	Males.		Fem.	Age.	Male. Fem. Fec. Tab.	
	Marr.	Fec.	Fec.		Marr.	Fec.	Fec.		Marr.	Fec.	Fec.			
15	...	...	22	28	82	80	96	41	60	51	51	54	30	5
16	...	31	44	29	87	70	94	42	...	49	48	55	29	...
17	...	36	54	30	93	60	92	43	...	47	45	56	28	...
18	...	40	63	31	100	51	90	44	...	45	42	57	28	...
19	...	45	73	32	95	51	86	45	...	43	39	58	27	...
20	...	49	82	33	91	51	82	46	...	41	37	59	26	...
21	...	54	92	34	87	51	78	47	...	39	34	60	26	...
22	47	61	97	35	84	51	74	48	...	37	31	61	25	...
23	53	70	97	36	80	51	70	49	...	35	28	62	19	...
24	59	80	98	37	76	51	66	50	...	33	26	63	12	...
25	65	90	99	38	72	51	62	51	...	32	23	64	6	...
26	70	100	100	39	68	51	58	52	...	31	17	65	...	...
27	76	90	98	40	64	51	54	53	...	30	11			

ABSOLUTE CONNUBIAL PROCREATIVE POWER.

The above table will not enable us to decide the procreative power of any one couple coming together, whose fixed ages are given; in other words, the United Procreative Power. Herein we are met with one great difficulty. The procreative power, which we have termed the *united* procreative power, is only to be deduced from the Irish Fecundity Tables. It bears a relation to the last table; but, with the data at our command, to determine this relation is impossible. The table for the year 1840, gives the number of children born for one year; and hence, *à priori*, we might hope that the result obtained would be conclusive; such, however, is not the case.

[*Table*

Table of United Procreative Power (Ireland) for 1840.

Age of Wife.	Age of Husband.						Remarks.
	Und. 17	17—25	26—35	36—45	46—55	Abov. 55	
Under 17	20	15	19*	50*	...	...	
17—25	40	44	47	44	35	20	* These numbers represent the number of children born to 100 marriages of each age.
26—35	...	45	48	42	28	22	
36—45	...	43	23	22	26	15	
46—55	...	50	20	...	3	8	
Above 55	...	...	...	...	...	...	

But very little consideration of the above table will show, that to deduce from it the procreative power of other than the middle ages, would be impossible; because, there being but very few and sometimes no marriages in the extreme periods, for obvious reasons, the cypher of fecundity for these periods cannot be depended upon. It is either much too high, or absent altogether, nor does any method for taking a mean in such cases, promise any very correct result.

On analysing, for instance, more closely, the above table for 1840, the power is found, by calculation, to be greatest at the ages of twenty-nine for males, and twenty-six for females. It would be reasonable to suppose, that the maximum age of united power would be the same as that for average physiological power. This is the truth in the case of females, the age being in both cases twenty-six; but it is not true for males. The reason has already been stated. The Irish Fecundity Table for 1840, is defective for the period under seventeen for males; and the cypher of a man's fecundity for the age under seventeen, enters into the calculation necessary to bring out the average maximum age of procreative power for males; while it is excluded in the calculation required for the maximum age of the united power. This accounts for the remarkably low maximum, obtained in the former case. Again, for the ages between thirty-six and forty-five, there is another defect, as it would appear that a man of that age has more procreative power than one of between twenty-six and thirty-five. This may arise from the table giving too small a number, for the fecundity of husbands of the former period. It is in reality, highly probable, as the subsequent reasoning will show, that the fecundity of these two periods is very nearly the same in both cases. If we attempt to raise the cypher of fecundity for the period between twenty-six and thirty-five, supposing that between thirty-six and forty-five to be correct, it must be raised to more than twice its present value, to make the maximum age of procreative power for males twenty-seven. These sources of fallacy, therefore, so entirely vitiate the result, that it was found impossible to make use of the fecundity table for 1840 singly, and the following plan was therefore adopted.

In the Census Tables of Ireland for 1841, the table given at page 487, was selected. The number of marriages, and children born to such marriages, were here given for the eleven years, from 1830 to 1840 inclusive. The number of children born to each couple was divided by the number of years they had been married; and the quotient in each case was supposed to indicate the number of children born to each couple per

annum. In this way, a series of additional numbers were obtained, for comparison with the numbers for 1840.

Had we been satisfied with taking the mean of the numbers so obtained, this proceeding would have been open to the objection, that fecundity is greater in the middle, than in the earlier or the later ages. The cypher of fecundity for the earlier ages would be too high, as the couples would, in these eleven years, be passing through the phases of greater fecundity pertaining to the middle ages, while that for the later ages would be too low, for precisely the converse reason. To take such a mean would, therefore, have been wrong.

Now it is a mathematical theorem that, in any curve, the mean of any number of ordinates, between two fixed points, is nearly equal to the middle ordinate; and differs from it less and less as the two points approach one another. This is most true, when the curve increases very rapidly, and is accurately true for the straight line. Applying this law to the present case, the mean number of children obtained between any two fixed ages by a large number of persons, will be nearly equal to the number of children obtained by that same number of persons in the middle age between the two. This will become more evident by an example. Supposing 27 be the average number of children born to 100 couples, both under seventeen, in eleven years; 27 may be taken as the measure of the fecundity of a couple aged twenty-one, nearly. As each male and female grow older each year, these results come to be arranged in diagonals in the table.

For the sake of greater accuracy, we have reduced this table to one for every two years, by taking means. Moreover, by comparison, the extraordinary and accidental numbers have been corrected; in an arbitrary manner, it is true, yet yielding necessarily results more to be depended upon, and probably sufficiently correct for all practical purposes. The intermediate years have either been filled up in a similar manner, or proportional parts taken.

There are yet two other sources of error, which, in strict honesty, should be here mentioned. 1. The three last years, 1838-39-40, are unfortunate years; as, whatever may have been the cause, there were proportionally fewer marriages in those years than in the preceding. This accident diminishes the value of our table, for the reasoning would be most correct for the later and least so for the earlier; and, 2. the results from the preceding years are formed from better data. These two errors mutually aid one another in vitiating the results.

No method, however, to obviate this inconvenience could be found; and hence, the results must be considered as unfavourably influenced, though probably not sufficiently to affect their practical utility. (See Table, p. 258.)

It is clear, however, that although the table (p. 259) may be looked upon as expressing the relative fecundity of different ages, among united or married persons of both sexes, abstractedly considered, still, it does not express the totality of procreative power, which we have termed the Connubial Absolute Power. This must depend on two other circumstances, first, the chance a person has of living through the succeeding ages; and secondly, of enjoying, in addition to his actual present fecundity, the fecundity appertaining to these succeeding ages.

Cyphers of Fecundity for 11 *years, ending* 1840.

Age of Wives.	Age of Husbands.						Age of Wives.	Age of Husbands.						Year.
	Und. 17	17 to 25	26 to 35	36 to 45	46 to 55	Abov. 55		Und. 17	17 to 25	26 to 35	36 to 45	46 to 55	Abov. 55	
Under 17	20	50	19	50	...	...	36 to 45	...	43	23	22	26	15	1840
	33	30	34	16	...	...		...	22	29	28	23	14	1839
	18	32	28	23	...	...		...	18	23	23	21	9	1838
	36	35	35	37	37	...		...	22	23	21	15	10	1837
	37	36	34	34	30	5		...	18	17	13	12	4	1836
	37	37	36	36	13	22		...	18	17	17	13	11	1835
	33	36	38	39	35	7		...	17	15	12	10	11	1834
	33	36	36	32	26	...		...	18	16	15	10	2	1833
	30	35	36	34	26	44		...	16	17	15	10	4	1832
	29	36	37	37	30	30		...	17	14	14	8	4	1831
	27	25	34	34	24	22		...	13	12	13	9	5	1830
17 to 25	40	44	47	44	35	20	46 to 55	...	50	20	...	3	8	1840
	21	43	45	46	43	42		...	16	6	4	6	...	1839
	35	42	44	43	40	25		...	17	8	8	4	1	1838
	37	41	43	42	37	41		...	...	5	7	4	...	1837
	37	41	42	41	34	30		...	...	15	3	4	...	1836
	43	40	41	39	36	22		...	...	13	1	6	2	1835
	35	39	40	38	35	26		...	58	6	8	4	2	1834
	32	39	40	37	27	22		...	4	5	5	2	...	1833
	33	38	39	37	35	11		...	18	9	5	3	4	1832
	31	38	39	39	30	24		...	4	8	8	2	2	1831
	28	37	38	35	29	27		...	4	6	9	4	1	1830
26 to 35	...	45	48	42	28	22	Above 55	...	...	...	...	...	...	1840
	62	41	45	47	36	32		...	...	...	25	1	6	1839
	22	39	43	40	38	25		...	...	...	...	8	3	1838
	35	38	43	39	33	28		...	...	...	...	...	...	1837
	40	36	42	34	31	26		...	...	...	...	...	...	1836
	22	36	40	34	26	22		...	...	...	...	...	2	1835
	27	35	40	32	27	19		...	...	...	...	...	7	1834
	27	34	40	33	26	19		...	...	...	8	...	2	1833
	23	33	39	30	26	7		...	...	...	...	...	4	1832
	23	32	39	34	24	14		...	...	...	...	3	9	1831
	29	30	38	27	23	13		...	...	...	...	...	...	1830

Both these conditions are regulated by his expectancy of life. The chance of living, should, therefore, also be taken in consideration, which will have the effect of raising, considerably, the numbers expressing physiological procreative power. The difference of this increase, is what we have before termed *Expectant Power*, and must be added to the connubial physiological, to give the connubial absolute power.

In drawing out this table, we have made use of the Carlisle Life Table, both because it is universally known, and because, even in these controversial times, it is very generally admitted to be good. It has the inconvenience of uniting the sexes; but, on the other hand, any inaccuracy hereby introduced, can scarcely much affect the result, and certainly not interfere with its *practical* utility.

It was necessary, however, to assume, in the calculations involved, first, that for so few a number of succeeding ages as ten, the procreative power increases or decreases, uniformly with the average rate between these limits; and secondly, that the same proposition was true, with regard to expectancy of life.

TABLE OF UNITED, OR CONNUBIAL PHYSIOLOGICAL PROCREATIVE POWER.[1]

Maximum Age, Males 33·94 ; *Females* 26·07 ; *Maximum Ordinate* 50.

Age of Wives.	Age of Husbands.																									
	16	17	18-19	20—1	22—3	24—5	26—7	28—9	30—1	32—3	34—5	36—7	38—9	40—1	42—3	44—5	46—7	48—9	50—1	52—3	54—5	56—7	58—9	60—1	62—3	64—5	66
16	40	44	48	52	56	57	57	58	59	56	54	51	48	46	40	34	28	22	16	13	11	9	7	4	2	0	0
17	42	52	54	54	59	64	65	66	67	68	66	63	59	56	54	49	46	42	38	34	24	14	12	8	4	0	0
18—19	45	53	55	56	62	67	72	73	74	75	74	71	66	65	64	62	57	53	48	44	40	28	17	12	7	0	0
20—21	50	56	57	58	66	70	75	80	94	95	94	90	89	88	84	73	72	71	70	63	56	50	35	20	9	0	0
22—23	52	62	64	66	68	74	80	86	95	96	95	93	90	88	86	76	76	76	76	76	72	59	46	33	10	0	0
24—25	54	63	74	75	76	78	83	88	96	97	98	97	93	89	84	76	75	72	70	68	66	61	52	44	23	0	0
26—27	56	64	71	77	80	82	84	89	97	100	97	96	92	87	83	78	74	70	69	66	63	58	51	43	22	0	0
28—29	58	66	72	79	84	86	88	90	97	97	95	92	90	86	82	76	72	68	62	59	56	53	50	42	21	0	0
30—31	60	66	72	77	82	85	87	91	96	92	90	88	86	84	81	74	68	60	54	52	49	46	43	40	20	0	0
32—33	55	62	65	69	73	72	75	79	82	86	86	85	83	81	80	70	63	56	49	42	40	35	30	25	20	0	0
34—35	50	52	56	61	65	70	66	69	72	76	80	77	73	70	67	64	62	54	46	40	38	33	28	23	12	0	0
36—37	45	46	48	50	56	61	67	64	67	70	73	76	72	69	66	63	60	52	44	38	32	26	24	22	11	0	0
38—39	40	41	42	45	49	53	58	63	62	63	64	66	68	64	60	57	54	50	42	37	29	25	22	21	10	0	0
40—41	35	36	37	38	42	46	51	55	60	60	59	58	57	56	53	50	46	43	40	36	26	24	21	20	9	0	0
42—43	30	31	32	33	34	36	38	41	43	46	46	45	44	43	42	40	39	38	36	34	23	21	18	14	10	0	0
44—45	24	25	26	28	30	32	33	34	35	36	37	36	34	33	32	30	28	26	24	22	20	17	15	8	4	0	0
46—47	19	20	21	23	25	27	29	30	30	31	32	28	28	27	27	26	26	24	22	20	18	16	14	8	4	0	0
48—49	14	15	16	17	19	22	25	27	28	29	28	26	24	24	23	22	21	20	19	18	17	15	14	7	3	0	0
50—51	8	9	10	11	13	16	19	21	24	25	26	24	21	18	18	17	16	15	14	12	12	10	8	6	2	0	0
52—53	3	5	6	7	8	8	10	12	14	16	17	17	16	15	14	14	13	12	11	10	9	8	4	2	0	0	0
54—55	1	2	2	3	3	4	4	6	8	10	12	11	10	9	8	7	7	7	6	6	6	4	2	1	0	0	0
56	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0

[1] The numbers represent twice the average number of children born to 100 couples, taking 100 as the maximum.

As regards the first supposition, it is, strictly speaking, not quite correct; but as the data, from which we have deduced the table of connubial or united power, are also imperfect, it is needless to make a more strict supposition in this case, lest we might lay ourselves open to the charge of pretending to accuracy which does not exist: because, as before remarked, two sources of fallacy cooperate to vitiate the result. Secondly, upon looking at the Carlisle table, it is found that the method of the straight line does not differ very much from the truth, for small periods, as exhibited in that table. If the straight line were accurately true, for any period of years, the same number of persons would die in that period every year. On looking at the table, this is found to be pretty nearly the case for periods of 10 years. Indeed, in the Northampton tables, of Mr. Price, this was so remarkably true, that (in 26 Geo. III., chap. 52) they were selected by Act of Parliament for estimating the legacy duty; and these tables were, and are now, almost universally used by the Life Assurance Offices.

In this manner, it was thought, an approximation, sufficiently near the truth, for all practical purposes, would be obtained. Indeed, on examining our data, it is doubtful if any other plan could have been adopted.

ABSOLUTE EXCONNUBIAL PROCREATIVE POWER.

The Absolute Exconnubial Procreative Power of any person, is his chance of bringing his dormant power into action. This may be realized in two ways; first, by the criminal connexion of the sexes, as in the production of illegitimate children; secondly, by the chance of a given person living long enough to marry, and thus begetting children.

The first of these, is the practical power of any unmarried person. To include, however, in the absolute power, this practical power, requires data which we do not possess. Tables should be given of the number of illegitimate children born during each period, as we have in the case of married persons, with the ages of both parents; and then, by drawing the proportion between these number of children, and the number of unmarried and widowed persons within procreative ages, we should obtain a cypher for this fecundity, which, though small as compared with that of married persons (since, for parents of all ages, the legitimate are to the illegitimate, as 16 to 1), might yet be appreciated, and by addition to this expectant exconnubial power, at once give the absolute exconnubial power.

In the absence of the data required, this practical power must be completely overlooked. While, therefore, we retain the term absolute exconnubial power, for convenience, it should be borne in mind, that this expression is incorrect, and that, in the totality given, the practical power having been omitted, the results are smaller than they should be in reality.

The second way in which a person may realize his dormant power, is by marrying in any one of the subsequent years of his life, and thus becoming possessed of connubial absolute power. The chance, therefore, of his obtaining this power, must be the measure of his fecundity. Take the case of a male. As his chance will be different for every year, so we must consider each year separately. In any one of these.

TABLE OF CONNUBIAL ABSOLUTE POWER.

(The totality of Physiological, Expectant, and Practical Procreative Powers.)

Age of Wives.	Age of Husbands.																										
	16	17	18-19	20—1	22—3	24—5	26—7	28—9	30—1	32—3	34—5	36—7	38—9	40—1	42—3	44—5	46—7	48—9	50—1	52—3	54—5	56—7	58—9	60—1	62—3	64—5	66
16	1664	1658	1652	1646	1640	1635	1528	1421	1314	1207	1100	994	947	901	854	808	762	597	432	267	102	81	61	40	20	10	0
17	1546	1658	1641	1624	1610	1627	1544	1502	1360	1219	1120	1030	949	884	838	791	742	594	445	296	194	92	89	47	25	12	0
18—19	1428	1533	1639	1580	1552	1577	1602	1628	1436	1245	1150	1054	958	862	806	759	708	650	541	352	262	172	117	55	30	15	0
20—21	1310	1386	1451	1537	1543	1549	1555	1563	1462	1270	1170	1072	973	874	775	723	672	614	557	408	314	230	146	62	36	18	0
22—23	1292	1328	1364	1400	1408	1416	1424	1432	1324	1296	1187	1078	969	860	751	687	636	593	550	464	336	288	208	80	42	21	0
24—25	1084	1133	1193	1252	1312	1334	1303	1272	1209	1209	1130	1028	927	826	725	662	600	544	487	431	371	280	189	98	59	20	0
26—27	955	1041	1090	1139	1188	1210	1233	1129	1125	1121	1012	1004	903	802	701	662	564	509	453	398	338	278	197	116	77	39	0
28—29	837	917	1008	1036	1064	1075	1087	1109	1071	1033	981	929	878	776	673	618	552	476	421	366	318	269	221	132	88	44	0
30—31	718	782	846	911	940	954	967	981	995	945	896	848	800	752	650	584	519	453	388	333	291	249	207	166	99	49	0
32—33	600	654	708	762	816	824	832	840	849	858	810	764	718	672	626	561	496	431	366	301	262	224	185	147	111	57	0
34—35	482	540	598	656	714	720	729	738	748	757	721	680	640	601	561	500	444	388	331	275	213	185	157	129	92	55	0
36—37	396	451	471	492	613	619	625	633	641	658	621	584	554	525	496	457	418	359	299	249	187	126	133	111	74	37	0
38—39	311	366	422	467	512	518	524	530	544	559	521	484	447	439	432	406	368	336	284	223	184	143	101	93	62	31	0
40—41	225	272	349	366	411	419	428	436	445	460	438	417	396	375	367	338	310	282	254	197	163	130	96	75	56	29	0
42—43	140	182	225	267	310	320	330	340	350	361	349	337	326	314	303	276	250	224	198	172	145	119	92	66	50	27	0
44—45	109	116	159	202	246	254	266	278	290	303	276	267	258	248	241	232	204	176	147	118	90	78	66	55	40	25	0
46—47	77	86	95	139	183	190	198	214	230	245	213	192	186	179	172	167	161	143	126	107	90	72	59	46	30	15	0
48—49	45	56	67	78	120	127	134	142	164	187	160	134	108	104	100	97	93	90	82	75	68	61	54	35	23	11	0
50—51	14	24	35	46	57	69	81	93	106	129	117	105	93	81	78	75	72	70	67	59	50	42	34	26	17	9	0
52—53	5	7	17	28	38	39	47	55	63	71	67	64	60	57	54	52	50	48	46	45	39	33	28	23	14	6	0
54—55	2	3	5	12	19	22	25	28	31	35	34	32	31	29	28	27	26	25	24	23	22	17	12	8	4	2	0
56	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0

The numbers here given, divided by 200, will give the number of children any couple, on the average, may be expected to get.

a male may marry a female of any age; his power, then, depending upon this, may be found by taking the chance of his marrying a wife of any age in a given year, and multiplying by his absolute connubial power, if he did so marry. The sum of these results gives his procreative power, depending on the chance of his marrying that year. Repeating this process for every succeeding year (or period of years) of his life, and taking into account his chance of life, the sum of these results will be his absolute exconnubial power for that year or period of years.

By the Irish Marriage Tables, these may be found for the years 16, 21, 31, 41, 61; and by taking proportional parts for the intervals, the tables may be calculated.

It should be, moreover, borne in mind, that as children have, necessarily, the chance of attaining procreative and marriageable ages, so have they also an amount of exconnubial absolute power. It is small, it is true, but still not to be neglected.

Taking these several circumstances into consideration, the following table was calculated.

Table of Exconnubial Absolute Procreative Power.[1]

Age.	Males.	Females.	Age.	Males.	Females.	Age.	Males.	Females.
0— 1	498	366	22—23	664	451	44—45	41	6
2— 3	611	445	24—25	557	394	46—47	37	5
4— 5	660	480	26—27	450	337	48—49	33	3
6— 7	700	510	28—29	343	279	50—51	30	2
8— 9	710	520	30—31	236	222	52—53	26	·01
10—11	728	530	32—33	129	165	54—55	24	.005
12—13	733	534	34—35	112	134	56—57	22	...
14—15	742	540	36—37	96	102	58—59	21	...
16—17	741	538	38—39	79	71	60—61	19	...
18—19	713	505	40—41	62	39	62—63	18	...
20—21	688	478	42—43	45	8	64—65	9	...
						66	...	...

The preceding investigation would enable us to give an opinion on several most important probabilities connected with our social relations in life. Two only, however, will here be noted. If a party presented himself before us, as being very desirous of offspring, we would be enabled to tell him, in relation to his own age, what ought to be the age of the partner to be selected, which would promise best for the gratification of this wish. In like manner, many other prudential considerations in social life might hereby be more certainly assured. It is to emigration societies, and governments, however, that tables, setting forth the procreative power for the various ages, prove most valuable. To the former, if the benefit and increase of the population in the colony be the chief desire, these tables at once explain at what ages persons should be sent out. To the latter, they designate those agents, who, as most active in the increase of an over-crowded population, should be removed.

If we are not misinformed, the principle adopted by emigration societies, is to send out no parties free of expense above twenty-five years old; and generally, they encourage persons of or about this age to

[1] The numbers here given, divided by 200, will give the number of children each person on the average may be expected to have.

emigrate. This plan may be correct, if the parties be already active procreative agents, or married persons; because they are very near the maximum age of fecundity, and, besides their bodily strength being greatest in and about these ages, these persons are more likely to be useful members in the colony: but if they be unmarried, and due regard be also paid to the future prospects of these persons, and to the increase of population in the colony, the value of such persons is not so great as that of children between two and three years old. This assertion may appear a startling one; yet it is borne out fully by the table of exconnubial absolute power before given.

These tables would, indeed, seem to point out the advantages to be derived in sending out persons upon the principle of a sliding scale; parties between twelve and seventeen being sent out free, and those either below or above these ages being charged at a rate proportional to their procreating value. In like manner, it explains the advantage there would be in sending out married persons at a cheaper rate than the unmarried, etc. The government, in seconding such a movement, would contribute powerfully towards ameliorating the condition of thousands, whose distressed social condition calls for such heavy taxation on the middling and wealthier classes of the community, and who, as the most active procreative agents (for who are more so than the poor?) promise tens of thousands more in equal misery, which the coming generations will have hereafter to support. All these persons, if removed and transferred to the colony, would not only benefit themselves, and assist towards the development of its resources, but likewise do great good to our own country, by consuming many of our useless manufactures, and checking, by their absence, the impetus of population at home.

The discussion, however, of this question, does not immediately concern us at present; though, as illustrative of the importance of the present inquiry, the digression may be excused.

The preceding investigation has necessarily occupied much labour and time. As it will be evident, our chief difficulty has been the imperfect nature of our data; yet is it a question that could, with proper data, be made most easy of solution.

First, as regards public or state documents.

1. If, in the population returns, there were tables, in which the exact age of parties marrying were separately given for every year, these results, when equated, would at once enable us to form another table for the expectancy of marriage for both sexes.

2. If in the registry of births, due note were made of the exact age of the parents, the legitimate being distinguished from the illegitimate children, a table of exconnubial and connubial practical power could be easily made out, and the maximum age of fecundity for both sexes at once determined.

3. The number of married, unmarried, and widowed, for every separate year, should be given.

4. In the Criminal Tables, the offences arising from sexual desire, throughout the country, should be given also for every year. This would also lead to the determination of the age of greatest procreative power.

Secondly, as to the collection of tables from private sources. There

are many of the greatest value, which medical men alone would be competent to provide.

1. The age at which puberty appears in a male. Hitherto no tables showing this have been made.

2. More extended tables of the date of the disappearance of the catamenial function in the female; distinguishing, in those cases where it has persisted late through life, whether the discharge *per vaginam* be or be not connected with uterine disease.

3. In the records of obstetric practice, more especially in hospitals and dispensaries, where patients present themselves for lying-in letters, and births or miscarriages occur, to note not only the age of the mother, as is almost invariably done, but also the age of the father.

These data given, the limits of the procreative ages, and the ages of greatest fecundity, could be very easily deduced.

In the absence of these data, our conclusions are less certain; yet, so far as the results above given go, the following General Conclusions may be drawn.

1. The age of greatest fecundity in males is from thirty-one to thirty-three, in females twenty-six.

2. But the procreative value of a person between fifteen and sixteen, is greater than at any preceding or succeeding age.

3. The ages at which most marriages occur, are about the same as the ages of greatest fecundity. Thus in the Irish marriage tables, in which only the proportion to unmarried and widowed could be drawn, the ages were, thirty-two for males, twenty-six for females.

4. There is, very probably, a relation between procreative power, and marriages taking place, at all ages.

5. Sexual desire and fecundity, appear to have also about the same maximum age, and bear a relation generally one to another. The English table gives a mean of twenty-eight for persons. The Irish twenty eight; the Scotch twenty-nine for males—both twenty-six for females. The fecundity for persons, according to the Irish, is also twenty-nine

6. And lastly. The earliest and latest limits of procreative power are sixteen and sixty-six for males, fifteen and fifty-five for females.

Such are the results which the preceding inquiry has brought out. It should be borne in mind, that perfect accuracy with our data was impossible. We have, therefore, endeavoured only to treat the question in its practical bearings; and in this view we venture to offer it to the profession. Hereafter, if better data be afforded, we may be more fortunate, or others may bring out more correct results. For the present, however, if we have contributed in the least towards the elucidation of this question, we must be satisfied.

19, Dorset Square, January 1850.

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ON THE ADMINISTRATION OF CHLOROFORM IN MIDWIFERY, AND AS A SEDATIVE OF UTERINE PAIN GENERALLY.

By J. HENRY BENNET, M.D., Physician-Accoucheur to the Western General Dispensary, etc.

In February 1848, I published, in the *Lancet*, the result of my experience respecting the administration of Chloroform in irregular and operative labour, in dysmenorrhœa, and in uterine pain generally; and on that occasion I stated that it had proved, in my hands, a most valuable therapeutic agent. During the two years which have since elapsed, I have been constantly in the habit of administering Chloroform in these varied conditions, and am able to confirm, in every respect, the statements which I then made; no single event having occurred in my practice, calculated to modify the opinion which I then expressed, respecting its great practical value to the accoucheur and to the uterine pathologist.

Various fatal cases, however, have been since published, in which death was so evidently the result of the employment of Chloroform, that we cannot but admit its inhalation to be attended with a certain amount of risk. That this risk is very slight indeed, is proved by the very small number of fatal cases recorded, as compared with the innumerable instances in which it has been administered for surgical purposes; and by the fact that, in many large hospitals in London and Paris, it has been given, ever since its first introduction, in all cases of operation, to the full anæsthetic extent, without any untoward event having been observed. Such being the case, I do not think that the fatal instances that have occurred, imply such an amount of danger as to induce us to forego the benefit of Chloroform, when it is likely to be therapeutically useful, or when its inhalation will save a patient from the pain of an operation; or even to refuse our sanction to its inhalation in a *slight* operation, if the patient shrink from any pain, and is willing to incur the very trifling risk attached to its administration. The very greatest care, however, ought clearly to be shown, and I cannot too decidedly urge the precaution on which I laid so much stress two years ago,—viz, that of seeing that the Chloroform vapour is mixed with a sufficient quantity of atmospheric air for the purposes of respiration; so that, on the one hand, asphyxia may not be produced; and that, on the other, the sedative effect of the Chloroform on the nervous centres may take place gradually, so that the latter may not be instantaneously paralysed by the *sudden* presence of a large quantity of Chloroform in the system. It is for this reason that, like Dr. Simpson, I generally prefer, for inhaling, a thin cambric handkerchief, to the various inhalers now in use. The former appears to me to admit more easily of gradual, progressive chloroformization. Extreme caution in the first stage of inhalation is the more necessary, as, in most of the cases of death from Chloroform that have been recorded, it seems to have taken place after a few inhalations only, and from a quantity of Chloroform, which appears incredibly small (half a drachm, or a drachm) to

those who are in the habit of using it, who continually find it necessary to give four or five times that amount, in order to produce complete anæsthesia.

Within the last eighteen months, I have myself taken Chloroform to the full surgical extent, on three occasions, with perfect success; twice for dental operations, and once for the extirpation, by Mr. Fergusson, of a fatty tumour from the shoulder. On each of these occasions, but especially on the two first, when a less perfect inhaler was used, I distinctly and painfully felt the sense of suffocation occasioned by the absence of a due supply of atmospheric air. This error is the more dangerous, as all sense of the wants of the economy is lost as soon as anæsthesia commences, and the patient may then be asphyxiated by the absence of oxygen, without any sense of suffocation being experienced or manifested.

I still administer Chloroform in four classes of cases. I. In Irregular but Natural Labour. II. In Operative Labour. III. To facilitate Operations on the Uterus. IV. To subdue Uterine Pain in dysmenorrhœa, etc.

I. In Natural Labour, I seldom resort to the inhalation of Chloroform, unless it be occasionally to facilitate its last stage, or to allay some morbid conditions of the nervous and circulatory systems, which may be interfering with the progress of parturition. The pains of labour are sometimes partially arrested, diminished, or rendered irregular and inefficient, by fear, impatience, want of self-control, or by long-continued suffering. When this is the case, Chloroform generally exercises an all-but-magical effect. Under its influence, all nervous excitement is soothed, the pains gradually become more regular, powerful, and efficient, and the labour once more progresses normally. This return of the labour pains, and their regularization under the calming influence exercised by Chloroform on the brain, probably explains the increase in their intensity, noticed by some writers, on its first administration; and thus is explained the apparent discrepancy of different observers, on which so much stress is laid by the opponents to Chloroform. If the pains are arrested or modified by nervous reaction, they return; if, on the contrary, they are too violent and prolonged, as is sometimes the case in the latter stages of parturition, when the soft parts are rigid and irritable, the intensity of the pains diminishes on the withdrawal of the excess of stimulation.

The disordered state of the circulation, and the congested condition of the capillaries, which are often observed in the cases to which I have alluded, also give way, in a most remarkable manner, to the calm produced by the inhalation of Chloroform. This is so much the case, that since I have had recourse to it, I have never found it necessary to bleed from the arm during labour; the inhalation of Chloroform having always been indirectly efficacious; even in some cases in which the cephalic congestion was so great as to make me fear convulsions.

Although I never hestitate, as will be perceived by the above remarks, to give Chloroform in natural labour, whenever I think it may be of use, I do not urge my patients to take it, merely as a remedy for pain. If they wish to avoid the pangs of parturition, and are anxious to inhale Chloroform (as I most certainly should, in their condition), I do not object to its administration. Firmly believing its careful inha-

lation to be innocuous in parturient women, I do not see how I can object; but I never press them to resort to it, not feeling called upon to interfere with a natural function, even to allay pain, if my patient be willing and able to bear it.

II. In Operative Midwifery, I continue to derive valuable assistance from Chloroform. The cases in which I have resorted to its use, have been, exclusively, turning and retained placenta. In consultation and dispensary practice, the accoucheur occasionally meets with instances, in which the placenta has been retained for some hours, before he sees the patient. In these, and in the more difficult forms of turning, Chloroform very much simplifies the operation, by rendering the patient unconscious of pain, and perhaps by slightly relaxing the rigidly-contracted uterus. In craniotomy, the pain inflicted on the mother is generally so slight, and the duration of the operation is often so prolonged, that the inhalation of Chloroform is scarcely, if at all, indicated. As I am also one of those accoucheurs who very seldom find the application of the forceps necessary, or even warranted, I have scarcely had an opportunity of judging as to the advisability of placing the patient under the influence of Chloroform, prior to their use. I should, however, feel inclined to give a negative opinion, as either the maternal parts are sufficiently roomy to admit of their application without pain, or they are not; and, in the latter case, the danger of injury to the mother would only be increased by her unconsciousness.

I have only once had to contend with post-partum hæmorrhage, after the administration of Chloroform, during or after labour; and in the case in question, the patient, the wife of a medical practitioner, was labouring under disease of the heart, unknown either to her husband or to myself. Her history is interesting in a scientific point of view, but very melancholy. She was a young, delicate, lymphatic female, twenty-three years of age; and I was called to her by her husband (August 1848), in the early stage of her first labour, owing to severe hæmorrhage having occurred. I detected a partial placenta presentation, and arrested the hæmorrhage by rupturing the membranes. The labour progressed favourably, but was rather tedious and painful during its latter stage, owing to the resistance and narrowness of the soft parts.

Under these circumstances, and with the entire approbation of the husband, she inhaled Chloroform, at intervals, for about an hour. Parturition was thus easily accomplished; and the placenta was expelled within a few minutes. The uterus contracted, the uterine bandage was properly applied, and all seemed to be going on normally, when, about ten minutes later, as I was washing my hands at the bed-side, I perceived, on looking at the patient, from the paleness of the lips and face, that hæmorrhage was probably taking place. I immediately unfastened the bandage, and, finding the uterus redilated, at once introduced the hand into its cavity, and found it filling with blood. With the left hand placed externally on the abdomen, I manipulated the uterus, to arouse its contractions; and, having passed the tube of an enema syringe, which was fortunately at hand, into the uterine cavity, freely injected cold water for some minutes. Under the influence of these means, the uterus contracted firmly on my hand, which I withdrew gradually, and no further hæmorrhage took place. I subse-

quently paid more than usual attention to this lady, as puerperal fever was then reigning epidemically; and I was fearful, lest the kneading of the uterus, to which I had been obliged to resort, might be followed by inflammatory reaction. Nothing of the sort, however, occurred; and all continued to progress favourably until the twelfth day, when she had an attack of indigestion, followed by fever, after taking a small quantity of animal food for the first time. She soon recovered, and appeared to be progressing so favourably, that, a few days later, I left her entirely to her husband's care. On the twenty-first day after her confinement, she again tried, in the afternoon, for the second time, animal food—a mutton chop; and, although she subsequently complained of considerable pain in the region of the stomach, she was cheerful and talkative during the evening. At eleven, her husband went into an adjoining room for a few minutes, but hearing a noise of choking, rushed to his wife's bed, and found her sitting up and gasping for breath. In less than a minute, she died in his arms, without having been able to utter a word. A careful *post-mortem* examination was made by my friend Mr. Avery and myself; but no morbid condition of any description was found in any region of the body, with the exception of the heart. This organ was enlarged, its cavities were dilated, and its walls softened. The uterus was perfectly healthy, and of a natural size.

In this case, death appears to have been the result of syncope occurring, under the immediate influence of laborious digestion, in a naturally delicate female, weakened by loss of blood, and labouring under dilatation of the cavities of the heart. It is well known to pathologists, that syncope is often attended with a fatal result, in softening and morbid dilatation of the heart. Indeed, in such cases, it is dangerous to bleed from the arm, to any great extent, lest fatal syncope should ensue, from the inability of the diseased and weakened heart to contract on the mass of blood which chokes its cavities.

The hæmorrhage which occurred after delivery, although not very considerable (for it was at once arrested), no doubt contributed to the subsequent fatal result, by increasing the debility of the patient; but that hæmorrhage can in no way be attributed to the use of the Chloroform. Post-partum hæmorrhage is one of the common accidents of midwifery, which every practical accoucheur meets with in women who have never taken Chloroform, and there is no evidence to prove that it is more frequent in those who have inhaled it. My own experience goes directly to negative such a proposition. This case is the only Chloroform one in which I have seen it, during the three years that I have administered the agent; whereas I have, during that time, been repeatedly called to cases in which it has occurred in patients, who had never inhaled Chloroform.

Moreover, females who labour under disease of the heart, are peculiarly liable to hæmorrhage after delivery. I recollect a remarkable illustration of this fact, which occurred in my wards, whilst I was *Interne* at the hospital of La Pitié, Paris. A woman, aged 34, multiparous, who was under treatment for heart disease (mitral insufficiency, hypertrophy, and dilatation), was found to be pregnant, and was consequently retained in the hospital for her confinement. One morning I left her

in perfect health, at ten o'clock A.M., and found her dead on my return, at four P.M. Labour had come on, and the child and placenta were expelled in a few hours; but, immediately after the expulsion of the placenta, irrestrainable hæmorrhage followed, and she died in a few minutes. The congested state of the venous system, and especially of the portal veins, explains this tendency to uterine hæmorrhage, in females in whom the circulation is impeded by insufficient heart-action.

I would here remark, that there is a very unphilosophical tendency in the opponents of Chloroform, to attribute any accident that may occur to parturient females, to whom it is administered, to the action of Chloroform, as if a certain proportion of accidents did not occur in midwifery practice, whether Chloroform be used or not. Thus I have heard it argued, that Chloroform may lead to puerperal mania, because Dr. Webster stated, at the Westminster Society, when Dr. Murphy's valuable paper on Chloroform was read,[1] that he had seen three cases of puerperal mania, at Bethlem Hospital, in which Chloroform had been administered. The question is, whether these females would not have been equally seized with puerperal mania had they not taken Chloroform? If it could be shewn, that since the use of Chloroform in midwifery had become more frequent, the annual number of cases of puerperal mania received at this hospital had very greatly increased, the argument would then be valid: but in the form in which Dr. Webster notices the fact, it is altogether valueless.

When administering Chloroform in simple midwifery, I never give it to such an extent as to annihilate pain, but merely so as to allay or deaden it; in a word, I do not attempt to render the patient totally unconscious of her sufferings, but merely to render them bearable. In operative midwifery, the anæsthesia must necessarily be pushed further, that is, to the surgical extent, as the intention is then both to render the patient unconscious of what is done, and to obtain the muscular relaxation to which this stage of anæsthesia gives rise.

III. In Uterine Examinations and Operations, Chloroform is occasionally very useful to me; though, as a rule, no instrumental examination of the uterus need be painful, if an instrument proportioned to the degree of dilatability of the vaginal orifice be used, and proper gentleness be observed. There are cases, however, in which the fears of the patient are so great, that the use of Chloroform may be desirable; there also cases in which the uterus, its neck, and even the vagina, are the seat of such intense neuralgic sensibility, that interference of any kind is all but impossible, unless the patient be under the influence of Chloroform.

I have now under my care a case of this kind, a married lady, aged 26, who for the last four years has been a martyr to neuralgia of the most intense and agonising description. The neuralgic affection has occupied principally the branches of the fifth pair, the nerves of the uterus, and occasionally those of other parts of the body, and has evidently been connected all along with extensive ulcerative disease of the cervix uteri. The uterine disease appears to have followed an abortion some years ago, and had been totally overlooked until she consulted me. The

[1] See London Journal of Medicine, vol. for 1849, p. 1084, 1170, for the discussion on, and an abstract of, Dr. Murphy's essay: vide also vol. for 1850, p. 90.

entire pharmacopœia had been ransacked, and the most powerful sedatives had been administered for years, without any favourable result. A careful digital examination of the uterus having brought on a frightful neuralgic attack which lasted a fortnight, I determined to put her under the influence of Chloroform, and thus was able to bring to light a very extensive ulceration of the cervix. This organ was lacerated, and the seat of great inflammatory enlargement. I have now given this lady Chloroform to the full surgical extent for fourteen consecutive weeks, that is fourteen times, and she has seldom taken less than half an ounce on a handkerchief, and sometimes more. I was very unwilling thus to continue its administration; but, until within the last week or two, she has never been able to bear the slightest interference whilst conscious, not even an injection of warm water. The disease, however, is now all but cured, and the morbid sensitiveness is fast disappearing. She has had no regular neuralgic attack for the last two months, and will evidently get quite well, both of the local disease and of the neuralgia which it occasioned. I have not been able to discover, in this patient's mental or general state, any detrimental effect whatever from this lengthened administration of Chloroform. It occasionally gives rise to sickness, but that is all: nor does there appear to be any difference in her subsequent condition, whether she have taken much or little; the quantity inhaled in order to produce the same anæsthetic effect varies, without our being able to discover the cause. Sometimes she becomes insensible after a couple of minutes' inhalation; sometimes after eight or ten only. This case appears to me to prove the general innocuity of Chloroform, even when taken in large doses and for a continuance. The last time it was administered, it was for the application of leeches to bring on menstruation, or to supply its deficiency, the menses being delayed to her great distress; and although she was kept under its influence for half-an-hour, there was even less subsequent distress from sickness than usual.

IV. In Uterine Pain from dysmenorrhœa, cauterisation, or from any other cause, Chloroform continues to prove, in my hands, a valuable medicinal agent, either when given by inhalation, or internally as a medicine. It is, however, capricious in its action, and not to be depended upon in every case, especially when administered internally; its virtues as a sedative of pain are also much more decided when inhaled than when taken. The most propitious moment for inhalation with this view, but the most inconvenient to the medical practitioner, is at bed-time; it will often insure freedom from pain, and a night's rest, when every thing else fails. For this effect to be obtained, however, the patient must remain perfectly quiet on recovering consciousness; and this is the reason that night is so much preferable to day for inhalation. If the patient who has inhaled Chloroform be once thoroughly roused, the sedative effect frequently passes away.

When given internally, twenty, thirty, or forty minims of Chloroform should be mixed with mucilage, or with the yolk of an egg, previous to water being added, so as to be held in suspension. If a little camphor be added to the Chloroform, which soon dissolves it, the solution will also mingle with water.

Chloroform may be injected into the rectum in any of these shapes, and if it be retained, its sedative effect is effectually produced; the rectal

mucous membrane, however, is so extremely sensitive, that it generally contracts, and expels the fluid containing the Chloroform.

In conclusion, I consider that my experience of Chloroform during the last two years, as likewise the published experience of others on the subject during that time, fully corroborates the opinions I formerly advanced respecting its great value, not merely as a means of lessening pain, but as a therapeutic remedy in connexion with its anæsthetic powers. I must also remark, that all that has been written against its cautious and exceptional use in midwifery, has been written by persons who had not used it, or at least not sufficiently to entitle them to give an opinion on the subject. Indeed, they appear merely to sustain a "thesis" founded on prejudice, and not to reason from their own experience.

At the same time, although thus supporting the use of Chloroform, I feel bound to repeat, that the fatal cases which have been published prove that its inhalation is not *altogether* free from danger to life. Neither does it appear that this danger can be avoided by the greatest caution; death having followed, as I have already stated, the administration of an insignificant dose in the hands of experienced practitioners. It would indeed seem as if, in the cases to which I allude, Chloroform had acted as a deadly poison from the first moment of inhalation. These cases must, therefore, teach us not to resort to it without a good and sufficient reason, and not to urge it on unwilling patients. For my own part, I am perfectly prepared in my own person, again to run the risk, such as it is, should I be again called upon to suffer a painful operation; and, consequently, I feel no hesitation in giving it to any one else who, like myself, may wish to avoid pain at a very trifling risk.

We must not forget, however, that the dangers of Chloroform inhalation have only been exemplified in surgical cases, and that no parturient woman has died during, or from its administration. They are, in reality, less exposed than surgical patients, as anæsthesia need not be carried so far; moreover, the entire economy is during labour in such an increased state of stimulation, that the vitality of the patient would, I think, be much less easily subdued, than in one who is depressed by the dread of a surgical operation.

9, Cambridge Square, Hyde Park,
January 1850.

BIBLIOGRAPHICAL RECORD.

RATIONAL MEDICINE: A VINDICATION. The Address delivered at the opening of the New School of Medicine, Surgeons' Hall, Edinburgh, November 6, 1849. By ALEXANDER WOOD, M.D., Fellow and Member of Council of the Royal College of Physicians of Edinburgh. pp. 88. Edinburgh: 1849.

We very cordially recommend this address to the notice of our readers. It is alike admirable in design and in execution, and we do not know any similar production which we could, with more satisfaction, place in the hands of the student. It has become the fashion with many, to speak in slighting terms of the appliances of our art; to represent cures as mere accidents, rather than the consequents of treatment; and, thus to encourage a spirit of sneering incredulity, which is, of all states of mind, the most unlikely to conduce to steady perseverance, and hopeful study. The little work before us will serve as an admirable antidote to such paralysing poison.

"Medicine," says Dr. Wood, "is at once a science and an art. But, as a living philosopher observes, 'Art is the application of knowledge to a practical end. If the knowledge be merely accumulated experience, the art is empirical; but if it be experience reasoned upon, and brought under general principles, it assumes a higher character, and becomes a scientific art.' (HERSCHEL.) This, then, is the great question we have to solve. Is our art really, as our enemies would fain represent it, a chaotic mass, without form and void,—where there is no general principle to guide, and no scientific rule to follow? or can we, on taking an impartial survey, really conclude, that, notwithstanding the numerous errors that have marred it, and the numerous imperfections that yet cling to it, there is still in it so much of truth as to bid us strive, amidst our imperfect knowledge, after that which is more full?"

Dr. Wood's object is to prove that the latter question must be answered in the affirmative: and this he does, not by slurring over the difficulties which beset the truthful inquirer, nor by denying the existence of many a broken link in the chain of our system; but by showing, that in the treatment of disease, there are certain fixed principles which serve as trustworthy guides—by demonstrating that, in the scientific choice of remedies, the philosophical practitioner is really influenced by the known conditions and necessities of the disordered economy. "It is in this," he observes, "that the true philosophy of the healing art consists. We must know that power which has become perverted; ascertain in what the perversion consists; endeavour to discover and remove the causes which have produced it; and, if the removal of these do not issue in the return of natural action, have recourse to means which may assist it so to do."

As an example, we quote his analysis of the treatment of inflammation. "If agreement among physicians, as to the plan of treatment to be pursued, were the test of the propriety of the practice adopted, assuredly we should not appeal to inflammation. One applies cold, another heat, to the part in which the disease is seated. One seeks to soothe it by emollient sedatives; another, to combat it by astringent stimulants. What a field for exultation to those, who delight to proclaim the uncertainty of medicine! How can practice so different be successful? If one is right, the other must assuredly be wrong. Such assertions, however, go no farther than to proclaim the ignorance of those who make them. In our management of disease, we possess few, if any specifics, which can directly destroy the morbid influence. We can only operate on it by modifying the vital force; and there are many agents, acting apparently in opposite ways, by which this purpose can be

effected. Thus, taking a very general view of the subject, we find that nervous irritation is usually the first step in the train of morbid phenomena exhibited by inflammation: and that it is followed by dilatation of the capillary vessels, with retarded motion of blood through them, and consequent increase in the quantity of the blood which they receive. Sooner or later, excitement of the heart supervenes, causing acceleration of the circulation throughout the body, and accompanied with changes in the character of the blood. Later still, we may have certain of the elements of the blood, or new products formed from it, deposited by those vessels in which the inflammation is seated. In endeavouring, then, so to regulate the vital force by which these derangements have been caused, as to lead the capillary vessels back to their normal state, it is evident, that respect must be had both to the stage at which the disease presents itself, and to the importance in the economy of the organ in which it is seated.

"If, for example, the morbid process have scarcely advanced further than the dilatation of the capillary vessels, then means may be employed to restore them to their normal state of tonicity. This may be done in two ways,—first, by allaying that nervous irritation from which the loss of tone first resulted, which may be effected by warmth, emollients, and other sedatives; or by exciting them to contract, by the use of astringents, or even of local stimulants. In the first case, the cause being removed, the effect spontaneously disappears. In the second case, means are employed to check the progress of the effect, irrespective altogether of the cause by which it was produced. The two classes of remedies which are thus employed, although apparently opposed in the way in which they act, may yet produce the same ultimate effect—operating, however, through different channels. The heat of the fire, which at first stimulated the nerves, and through them, produced inflammation of the tissue, may, if steadily applied, and longer continued, cure that inflammation which itself excited, by causing the disturbed capillaries to contract; or the apparently opposite remedy,—plunging the part in cold water,—will equally alleviate the disease. Seeming contradictions, certainly; but real contradictions only to those who refuse to study the vital laws.

"But, though one great object is to produce contraction of the capillary vessels, mere local treatment applied to them will not, in every case, suffice. The energetic action of the heart, forcibly propelling the blood into these vessels, acts by keeping them in a state of forced distension. By reducing the quantity of the circulating fluid, we remedy this in various ways; and hence the utility of bleeding in cases of acute inflammation. For, *first*, by removing some of the circulating fluid, we relieve the over distension at the affected part. *Second*, we diminish the blood, which is the normal stimulus of the heart, and thus weaken the force of the heart's action. *Third*, by withdrawing part of the blood from the brain, its susceptibility is diminished; and thus irritation is allayed, because the brain then less readily sympathises with the affected part, and also less powerfully stimulates the heart to increased action. *Fourth*, we cause an afflux of blood to the opening in the vein, and thus mechanically diminish the congestion at the seat of the disease. *Fifth*, we effect an alteration in the character of the circulating fluid, as is proved by the disappearance of the buffy coat of the blood after free bleeding in inflammation. There are certain medicinal agents also, although not so potent in their action, which produce similar effects. Tartar emetic, for example, at once diminishes the action of the heart, and causes contraction of the capillaries; and thus is entitled, perhaps, to rank next to bloodletting, in the treatment of inflammation. Saline substances, again, taken into the intestinal canal, withdraw water from the blood circulating in the adjacent vessels; and thus the quantity of that fluid is decreased, and its quality altered; hence they are largely used in the treatment of inflammation. Opium, again, diminishes the nervous sensibility, and thus removes

one element of the disease. There is no blind play in all this. The agents we have recourse to, have a determinate action ; and the determinate action of each is precisely suited to combat some one of the elements, of which this complex disease is formed."

In an appendix, Dr. Wood republishes, from the *Northern Journal of Medicine*, a review of Dr. Hastings' work on Naphtha, and a critique on the alleged mesmeric cure of Miss Martineau, both of which will repay perusal.

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1. Pathology of the Human Eye. By J. Dalrymple, F.R.C.S. Fasciculi III and IV. London : 1849.
2. A Treatise on the Inflammations of the Eye-ball. By Arthur Jacob, M.D., F.R.C.S. pp. 344. Dublin : 1849.

The promise of the early numbers of Mr. Dalrymple's work continues to be amply fulfilled.[1] The fidelity of the delineations of disease is only equalled by the exquisite beauty of the execution.

Part II presents us with excellent representations of the various Ulcerations, Tumours, and Warts, which have their seats in the eyelids, with diseases of the Lachrymal Apparatus, Ecchymosis of the Eye, and Scrofulous Ophthalmia. The figure 1, in plate VII, representing Ecchymosis, or a literally "blood-shot" eye, is admirable ; and the delicacy of execution of figures 5 and 6, in the same plate, is greatly to be commended.

Part III is devoted to Purulent Ophthalmia, as it appears in the infant and in the adult; and that formidable malady is most truthfully depicted from its commencement, through its various stages, to the sad and hopeless conditions which but too often result, even when treatment has been judiciously applied. There are also two clever and striking representations of a very peculiar and very terrible disease—apparently of carbuncular character —which occurred in the practice of that very eminent ophthalmic surgeon, Mr. Wilde, of Dublin. It has never fallen to our lot to meet with such a case.

Part IV cannot be sufficiently commended. It renders us familiar with Inflammation of the Conjunctiva, Ulcers, and Inflammation of the Cornea, with their sequelæ, and Sclerotitis. We would especially point out fig. 1, in plate XV, as a masterpiece of pictorial skill.

The value of the plates is greatly enhanced by the excellent instructions for treatment, which are laid down with simplicity and clearness ; and the work, when completed, will, in our judgment, be the noblest contribution to ophthalmic science, which this country has ever produced.

Dr. Jacob's remarkable work originally appeared in the form of detached essays, in the *Dublin Medical Press*, and is, therefore, we presume, generally known by the profession. For this reason, we leave our readers to judge for themselves as to the correctness of the views entertained by the learned author.

Contributions to Mental Pathology. With Introductory Observations, containing the Past and Present State of the Insane in Ceylon, etc. By James George Davey, Licentiate of the Royal College of Physicians, London, etc. 8vo., pp. 276. London : 1850.

The title of this work scarcely gives a correct idea of its contents. The "Contributions to Mental Pathology" form by far the smallest portion, the great bulk of the volume being occupied with an exposure of the unpleasantness of the author's position, as superintendent of the Lunatic Asylum at Ceylon—a vindication of his own conduct there, and loud complaints of the treatment he received at the hands of the medical authorities of the colony. We think Dr. Davey was ill-advised, in putting forth his statement in its present shape. A pamphlet, containing the essential facts, would have an-

[1] London Journal of Medicine for 1849, p. 356.

swered the purpose quite as well as this volume—nay, much better ; for any one who may buy the book, trusting to its title, will rather feel himself aggrieved, by finding he must wade through page after page of irrelevant matter, and vex his brain with the quarrels of Dr. Davey and the "P.C.M.O." The state of mind thus induced will scarcely be favourable to the reception of that impression, which the author has laboured to produce.

Dr. Davey is a keen supporter of the phrenological views, and a mesmerist to boot, as the following quotation will show :—"We feel we should be guilty of great and unpardonable neglect, and moreover incur, and deservedly too, the censure of the man of science and the philanthropist, if we were to allow this opportunity to pass without insisting on the remedial efficacy of animal magnetism, or mesmerism, in cases of epilepsy, combined with mental disease. Since our connexion with Hendelle, we have succeeded in restoring one poor native lad to perfect health, who, for a period of, we learn, seven years, was afflicted with epilepsy of the most distressing character. He used to have from two to five fits every day, and each one was commonly so severe as to leave him for some time more or less insensible. His appearance, when first mesmerised, was exactly that which belongs to the last stage of phthisis, so emaciated was he. Three months after we had commenced our operations, not only had the fits *entirely* left him, but he then presented the appearance of robust health, and had become stout, and really muscular. The case created a great sensation among the servants, whose remarks upon it were not a little curious. When on the eve of obtaining his discharge, he was attacked with apoplexy, and died about thirty hours after. A very similar case has occurred in our private practice ; and it affords us much pleasure and happiness, to learn of our patient's continued good health."

We do not stop to comment upon this extract, but leave each reader to judge of its value, in accordance with his state of knowledge, and his capacity for disentangling the opinions from the facts of an author.

A Treatise on the Practice of Medicine. By George B. Wood, M.D., Professor of Materia Medica and Pharmacy in the University of Pennsylvania, President of the College of Physicians of Philadelphia, etc. Second Edition. Two Vols. pp. 1622. Philadelphia: 1849.

The work before us, though not possessing much originality, gives evidence of great industry on the part of the author. He has made himself acquainted with almost all, if not all, the contributions which have been made to pathology and practical medicine, by the labours of American, British, and continental observers ; and has, with considerable ability, digested these, along with his own remarks, into a very creditable treatise. The first part of the work, amounting to 215 pages, is judiciously devoted to General Pathology and Therapeutics ; and here we find abundant evidence of his having drawn largely from the stores of pathological knowledge collected by the labours of Drs. C. J. B. Williams, A. B. Garrod, Hughes Bennett, Andral, Rokitansky, and others.

While we thus express our sense of the merits of Dr. Wood's work, the frequent references to the American editions of Dr. Williams's Principles of Medicine and other British works, lead us to express our opinion of the trade of *re-printing*, which is carried on so actively by our transatlantic brethren. In almost every American journal which we receive, we meet with advertisements of re-productions, often with astonishing rapidity, by American editors, not only of the standard works, which, from time to time, appear in this country, but even of many of small value. In the year preceding May 1849, it appears that twenty-five native medical works, including second editions, were published in America ; while thirty-six, principally English, were re-

produced.[1] Such proceedings both indicate and tend to perpetuate a low and spiritless condition of native American literature. That American physicians are capable of producing original works of the highest class, is undoubted; and of this proof has been given in our pages by our reproduction of the anatomical and physiological researches on cartilage of Dr. Joseph Leidy. Every day adds to the treasures of native American literature and science. We hope that our medical brethren of the great commonwealth beyond the Atlantic may soon cease to be contented to reprint English books, and that Americans may become as emulous to equal the mother country in literature, as they have shown themselves to be in arms and commerce. We are all children of the same family; and whatever greatness may be achieved by American citizens, will be a source of pleasure and peculiar pride to British subjects, their elder brethren.

Pathology of the Kidney in Scarlatina. Illustrated by Cases. By James Miller, M.D., Physician to the Western General Dispensary. pp. 177. London: 1850.

This is a valuable addition to the literature of Scarlatina; and deserves to be classed with the best monographs which have appeared on this disease. The cases are full of interest, and will, doubtless, be read with great profit by all who have the time and taste for such studies, and who do not rest satisfied with what is conveyed by a paragraph in reviews or retrospects. Dr. Miller corroborates the opinions of the best and most accepted authorities; and we think that those who have given adequate attention to Scarlatina will concur with most of his opinions. The subject has recently been discussed at such length by various authors in this Journal, that we cannot upon this occasion afford space to re-enter upon its consideration, nor to give any extracts.

Dr. Miller entertains the same views regarding albuminuria as have been expressed in this Journal, viz., that it is not pathognomonic of special renal disease, but has been long since proved to be only the depletion of a congested kidney—a serous hæmaturia. The engorgement may depend, primarily, on granular disease of the organ, on an aneurism or other tumour pressing upon the emulgent vein, on the pressure of the gravid uterus, etc.; but in all these cases albuminuria is simply the sign of congestion—the spontaneous depletion of a congested organ.

Though Dr. Miller does not pretend to give a complete bibliography, yet, from the aspect of that which he has produced, we think it was due to Dr. Snow, Dr. Behrend, of Erlangen, and others, to have noticed their essays. Dr. Snow's papers appeared in the *Lancet* for 1839 and 1843; and a full account of Dr. Behrend's paper was given in this Journal for 1849, p. 765. The author of such a monograph as that now under review, ought to show that he is conversant with the labours of his predecessors and contemporaries, and likewise that he is willing to give them as much credit as they deserve.

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[1] Transactions of American Medical Association, vol. ii, pp. 407, *et seq.*

CRITICAL DIGEST OF THE BRITISH AND FOREIGN MEDICAL JOURNALS.

PRACTICE OF MEDICINE AND PATHOLOGY.

ON THE SITE OF MORBID ACTION IN DISEASES OF BONE.

By Mr. John Goodsir, of Edinburgh.

I have, in another place, directed the attention of the physiologist to the important distinction which exists between the essential and accessory elements of a texture. "A texture may be considered either by itself, or in connexion with the parts which usually accompany it. These subsidiary parts may be entirely removed, without interfering with the anatomical constitution of the texture. It is essentially non-vascular, neither vessels nor nerves entering into its intimate structure. It possesses in itself those powers by which it is nourished, produces its kind, and performs the actions for which it is destined; the subsidiary or superadded parts supplying it with materials, which it appropriates by its own inherent powers, or connecting it in sympathetic and harmonious action with the other parts of the organism to which it belongs."[1]

The neglect of this distinction by the pathologist has been the cause of much misconception regarding morbid action in textures, and has introduced many errors into the general theory of disease. Disease may have its site either in the essential or accessory elements of a texture. Our knowledge of the diseases which have their site in the essential elements of the textures is still in its infancy; but I am inclined to believe, that they consist principally of alterations in the nutritive and reproductive functions, in the chemical or physical constitution of the textures. The fatty degeneration of the muscular texture is a familiar example of a morbid change of this kind.

The great majority of the recognized morbid changes have their site in the accessory elements of textures. They are situated in the midst of the areolar texture, and among the vascular and nervous networks which separate as well as connect all the essential textural elements of the frame. They consist generally of bloody or other infiltrations; of lymph, in various stages of development; of pus; of the various forms of new formation, such as cancer, tubercle, etc. It will thus be perceived, that the majority of diseases are situated, not in the essential textures of a part, but around or amongst them; that the majority of these are new formations, parasitic in their nature, making their appearance, undergoing development, and occupying a position in the areolæ of that general connecting texture, which, either in the form of the nucleated cellule, or of the filamentous fibre, is the first to appear, as well as the most lasting and abundant of all the textures.

The more distinct the conception, therefore, which an observer may possess of the relative position of the essential and accessory elements of any given texture, the better prepared will he be to investigate and understand the phenomena of diseased action in that texture.

The successful investigation of the site and phenomena of diseases in one texture, will render the investigation in all the others comparatively easy. I have been accustomed to consider the osseous texture as best adapted for this purpose, from its stability, and the comparative ease with which its essential and accessory parts may be separated from one another, distinguished, and examined. "A well-macerated bone is one of the most easily-made, and, at the same time, one of the most curious anatomical preparations: it is a perfect example of a texture completely isolated; the vessels, nerves, mem-

[1] Anatomical and Pathological Observations, p. 64.

branes, and fat, are all separated, and nothing is left but the non-vascular osseous substance."[1]

The essential elements of the osseous texture consists of all that part which resists maceration, along with the contents of the corpuscles. The accessory elements are the periosteum, with the vessels and nerves which ramify in it; the cells, areolar texture, fat vessels, and nerves, which occupy the continuous network of the Haversian canals and cancelli, which are hollowed out in the hard substance.

Morbid Changes in the Essential Element of Bone. In the course of lectures which I delivered in the theatre of the College of Surgeons, in the winter of 1842-3, I announced the existence, and hazarded a hypothesis as to the functions, of minute masses of nucleated particles, occupying the corpuscles of bone, which are generally believed to be empty spaces. I, at the same time, stated, that I had been led by my observations to consider it as highly probable that caries, or intractable ulceration of cancellated bony texture, consisted essentially in more or less complete obliteration of the corpuscles and their contents, as well as of the connecting canaliculi, and the neighbouring Haversian canals.

I have lately repeated some of my observations on these bodies, and have been confirmed in my belief of their importance in the economy of bone, and more especially of their obliteration in caries. Caries never attacks compact bone. It is met with only in shafts, when expanded by previous disease, in the epiphyses or extremities of the long bones, and in the short bones. It occupies surfaces somewhat limited in extent, irregularly hollow, with a surface hard and spicular, encroached upon, but not generally covered by, unhealthy granulations; the pus, discharged from which has, very generally, mixed with it, minute portions of bone, apparently thrown off from the carious surface. Caries is to be distinguished from the mere absorption of the surface of a bone, as well as from that open condition of the cancellated texture which is the result of the removal of the cartilaginous and osseous articular surface. These two latter conditions of bone are capable of cure, although slowly and with difficulty, partly in consequence of the disease which accompanies or precedes them, but principally because the cancellated texture contains few Haversian canals, the abundance of which, in the shafts of bones, explains the vigorous actions which they exhibit.

In true caries, it may be observed, that for a certain depth below the surface of the affected part, the corpuscles and canaliculi have more or less completely disappeared, so that the subjacent unaltered osseous texture is covered in by a layer of apparently homogeneous solid bone resembling marble. It is analogous, in some respects, to the enamel of the teeth—1, in not being covered by soft parts; 2, in being a portion of the exterior surface of the body; 3, in containing no corpuscles or canaliculi; 4, in being incapable of absorption, and requiring, therefore, to exfoliate, or to be removed artificially.

I am not prepared at present to enter upon the consideration of the changes which the essential element of bone undergoes in rickets, malacosteon, etc., but shall refer, in the meantime, as confirmatory of my own views and observations regarding the condition of the corpuscles and canaliculi in disease, to the recent work of Von Bibra and Geist.[2] In this work, which is principally devoted to the consideration of that peculiar necrosis, which attacks the jaws of the workmen engaged in making phosphoric matches, the authors have allotted a section to the microscopic structure of various forms of diseased bone. They have described and figured the empty condition and the altered form of the corpuscles, as well as the obliterated canaliculi in certain

[1] Anatomical and Pathological Observations, p. 64.

[2] Die Krankheiten der Arbeiter in den Phosphorzündholzfabriken, insbesondere das Leiden der Kieferknochen durch Phosphordämpfe, p. 77-86.

forms of exostosis and caries, without attaching apparently much importance to the observation.

II. Morbid Changes in the Accessory Parts of Bone. The greater number of the morbid changes in the accessory parts of bone occur during that condition usually called inflammatory.

As the accessory parts are situated on the external and internal surfaces, and in the Haversian canals[1] of bone, the changes which occur during inflammatory action make their appearance only in these situations.

The increased vascularity which the texture presents when inflamed, depends in the first place on the engorgement, and in the second, on the increase in number of the vessels which are situated in the periosteum, the medullary membrane, and the Haversian canals.

After the accession of inflammatory action, the surface of compact bone assumes a porous condition, and its substance becomes more or less cancellated. This change is induced by an action similar to that by which the cancelli of healthy bone are formed. The Haversian canals assume an increased calibre, so that their orifices on the exterior surface of the bone become larger, and their cavities ultimately open into one another laterally, and assume the appearance of cancelli.

As the Haversian canals increase in size, their cavities are filled, in the first place, by enlarged and new blood vessels ; and in the second, and principally, by a substance having a gelatinous appearance, similar to the lymph effused in other inflamed textures.

When the inflammation extends to, or occurs at, the external surface of a bone, the periosteum is raised by the effusion, dragging out or extending the processes, which stretch from its external surface into the superficial Haversian canals : and as the texture which occupies the canals is the formative organ of bone, these retracted processes are the centres from which new deposits of bone proceed.

When the action is acute, and occurs in certain constitutions, death of the inflamed bone may take place, before any very decided change has appeared in the Haversian canals.

But if the inflammation is more chronic, new bone is formed soon after, or along with the enlargement of the Haversian canals. This new bone is situated around the orifices of the canals, enclosing the processes which have been drawn out by the sub-periosteal effusion, so that the surface of an inflamed bone presents numerous orifices of enlarged and open Haversian canals, with their lips more or less thickened or everted ; the internal half of the convex edge, which bounds an orifice belonging to its own lip, the other half to those of the neighbouring canals. The inflamed surface is now slightly elevated by this new deposit of bone, the degree of elevation, and the size of the canals, being greatest at the centre of the inflamed spot, or where the action was most acute. This process of deposition continues for some time, and exhibits, on section, a structure similar to that of the subjacent inflamed old bone, the Haversian canals being large, and assuming more of a direction rectangular to the surface.

An enlargement by absorption now occurs in certain Haversian canals, in the new and old bone.

If the inflammation be periosteal,—that is, if it be confined to the superficial part of the bone, this secondary absorption takes place in the first formed canals of the new bone, those on its deep surface, and in those of the external aspect of the old. There now appears, in consequence of this absorption, a line of cancellated texture between the new and old bone. If the action have involved more or less of the entire thickness of the compact wall of the shaft, the secondary absorption invades, from the surface inwards,

[1] See Howship's *Observations on Healthy and Diseased Bone,* in vols. vi, vii, viii, and x, of the *Med.-Chir. Transactions,* London.

a greater or less extent of the old bone, and less of the new. While the inflammatory action and the process of absorption continues, new deposit of bone still appears towards the surface, increasing the swelling, and preserving the more compact structure of the periosteal aspect.

By modifications of this general mode of action, all the varieties of inflamed bone are induced.

A node is produced by an action such as has been now described, occurring over a limited portion of the periosteal surface, the inflammation and secondary absorption not extending, in general, deeply into the surface of the old bone, but confining themselves to the newly formed texture.

A section of a common osseous node, therefore, presents, from the external surface inwards, first its periosteal compact surface, the Haversian orifices of which, are larger than those of the healthy bone, and largest at the most prominent part of the node ; secondly, its cancellated texture, consisting of enlarged anastomosing irregular Haversian canals ; third, a layer of the external surface of the original bone cancellated,—that is, with its Haversian canals enlarged over an extent equal to the base of the node, and to a depth corresponding to the progress of the inflammatory action inwards.

In some nodes, the section exhibits the secondary absorption entirely confined to the new bone, so that the latter appears as if it had been entirely developed in the periosteum, and had not become intimately united to the surface of the old bone.

The uniform, or more or less irregular, thickening of the shafts of bones which have suffered from chronic inflammation of a specific character, is produced by modifications of the same process of deposition and absorption as occurs in the formation of nodes. The orifices of the Haversian canals on the surface of the expanded shafts are all more or less enlarged, especially where the action has been greatest, and the bone most prominent. They exhibit various modifications of the ring-shaped, or tubular osseous deposit which has been formed around, and encircles each of them ; and, according as these happen to be arranged or combined, we may observe the surface covered by tuberous, spicular, stalactitic, or obliquely-lamellar processes, the osseous edges and extremities of which represent the Haversian orifices with corresponding peculiarities of character and combination.

Sections of enlarged shafts exhibit, on a more extensive scale, the results of secondary absorption, similar to those which occur in nodes. When this absorption has been more or less completely confined to the new bone, the original wall of the shaft may be traced through the section, with more or less of its original thickness. When the entire thickness of the shaft has been affected, then the absorption will have extended over a greater or less extent of the compact texture of the original wall, and the entire bone will be light and cancellated.

In nodes and enlarged shafts, we occasionally observe alternate layers of compact and cancellated bone, more or less regularly disposed. This is one of those appearances, which have given rise to the expansion theory of enlarged bone. But as bone enlarges in disease, as in health, by reciprocal depositions and absorptions, this laminated arrangement cannot be explained by the hypothesis.

It would appear to be the result of renewed inflammation, nearly in the planes of the compact laminæ of the originally cancellated node or shaft, and a consequent deposit of new bone narrowing the canals, and rendering compact a plane of bone immediately exterior.

The spicular, nodular, or lamellar processes which are developed around ulcerated or carious surfaces, and at the margins of exfoliations, are produced in the same manner, by modifications of the Haversian canals of the part. The extreme modification of this kind of enlarged bone, is exhibited by the more or less complete osseous shells which envelope dead shafts, and the

origin, or source of which, have given rise to so much controversy in the pathology of necrosis.

The areolar and vascular texture contained in the Haversian canals of bone, is also the site of all cancerous, medullary, sarcomatous, fibrous, and scrofulous deposits, and tumours in bone. The essential element of the texture is in no way affected by these new formations. The cells, granules, and plasma of which they consist, become developed, and propagate themselves in the areolar and vascular net-works within the canals, like the sporules, thallus, and phycomater of fungi; the walls of the osseous canals giving way by absorption before the increasing mass of new organisation.

But here also, as in the inflammatory affection of bone, the absorption of one part is accompanied by deposition in another.

It is highly interesting to observe how the spicular and laminated masses which lie imbedded in cancerous and medullary tumours of bone, if traced back to the old bone from which they arise, will be found to spring from between the more enlarged Haversian orifices, out of which the parasitic masses have protruded; and how these spicular lamellated processes derive their characteristic forms and directions from this anatomical arrangement.

Ulceration of bone consists in the entire removal of the osseous texture of a part, by the continued and complete absorption of the walls of the Haversian canals of the part affected. It is important to observe, that this absorption or ulceration of bone never takes place, without a corresponding deposition of bone in the neighbourhood.

Conclusions. 1. That in the osseous, as well as in the other textures, we must distinguish between those morbid conditions which have their site in the essential, from those which are situated in the accessory, parts of the texture.

That, in reference to the changes in the essential parts of bone;

2. Caries, one of the most obstinate and painful affections of the texture, depends on the more or less complete obliteration of the corpuscles and canaliculi, and the conversion of the carious surface, for a certain depth, into a substance resembling the enamel of the teeth.

3. That all the inflammatory changes in bone have their site in, and depend on, an increase or diminution in the number and size of the Haversian canals.

4. That all the malignant and non-malignant growths in bone have their site in the Haversian canals.

5. That the various forms of spicular and laminated exostosis, which are found co-existing with malignant or non-malignant growths, are necessary results of the dilatation and extension of the affected Haversian canals. [*Edinburgh Monthly Journal*, February 1850.]

INDURATION OF THE CEREBRUM.

M. Duplay, physician to the Hospice des Incurables in Paris, related the following interesting case at a meeting of the Société de Biologie, in November 1849:

Case. V., aged 41, was admitted into the hospice for males, on the 22d of June, 1849. He had complete paralysis of the lower extremities, and moved the upper limbs with difficulty, so as not to be able to feed himself. His intellect appeared intact, for he understood all that was said to him; but he had extreme difficulty in articulating. His sister gave me the following account of the progress of his disorder:—

He had enjoyed good health up to 1839. He was strong, and well able to follow his occupation as a sawyer; he was not addicted to drinking, but appeared to have indulged in an abuse of venereal pleasures. In the course of the year 1839, he felt numbness in the lower limbs, which gradually became weak; walking was at first unsteady, and soon became so difficult that he

was obliged to use a stick, and subsequently crutches. The usual remedies were employed: the spine was covered with cauterics and moxas; he was admitted into the hospital St. Louis to have sulphur baths; but no remedies had any effect. At the end of three years, his speech became difficult, and soon almost unintelligible. He preserved his intellect entire. The rectum and bladder were paralysed; he had no stools without the aid of enemata or purgatives. About the time when the difficulty of speech appeared, the movements of the upper limbs began to be unsteady. Having exhausted all his resources, and obtaining no amelioration of his condition, he was admitted into the hospice.

On the 29th November, 1849, V. was admitted into the infirmary of the hospice with symptoms of pericarditis, and pleurisy of the left side. By energetic treatment, these affections were removed in a fortnight; but his health had been much shattered by them. In the sacral and trochanteric regions, there were large sloughs; he was in a state of extreme emaciation, and it seemed improbable that he would be able to bear up with the abundant suppuration. During his stay in the infirmary, I was able to observe the affections of the nervous system, which continued the same up to his death.

There was complete paralysis of the lower extremities; the legs were semi-flexed on the thighs, and the thighs on the pelvis; and they returned to this position immediately on the cessation of any attempt to straighten them. Sensation was preserved; the slightest irritation of the skin was felt by the patient. The upper limbs were not in a state of semi-flexion; the patient could move them, but was unable to regulate the movements, and easily let fall whatever he took in his hand; sensation was perfect here also. His speech was unintelligible; he could only with very great difficulty articulate a few words at intervals. The intellect appeared intact, and he understood the questions which were addressed to him. The functions of sight, hearing, smelling, and taste, were normal. The rectum was paralysed; and there was incontinence of urine. I was several times obliged to administer croton oil, in order to obtain alvine evacuations. The patient became gradually weaker, being reduced by the extensive suppuration, and died in an extreme stage of marasmus, on the 23d of November, 1849.

Post-mortem examination. The appearances found, were such as the progress of the disease had not led us to expect. The bones of the skull seemed natural; the dura mater was not adherent to the cranium, and the arachnoid cavity did not contain more serum than usual; the cerebral arachnoid presented, on the convex surface of the hemispheres, a slightly opaline tint; the sub-arachnoid cellular tissue was slightly infiltrated with a transparent serosity; the membranes were not at any point adherent to the cerebral convolutions, but were easily detached. The grey matter of the brain was normal; no change in consistence or in colour could be perceived. The outer third of the white matter on each side, except slightly presenting vascular points, was of normal consistence. But the two other thirds presented a remarkable increase of consistence, which gradually became greater towards the ventricles. The cerebral substance here was divided with difficulty; the slices resisted traction, and required considerable force to be used before the finger could divide and crush them. The change was symmetrical on the two sides, and occupied an extent of six centimètres (2·358 inches) from before backwards, by four (1·572 in.) in a transverse direction. The white substance had lost its brilliancy; it was dull, and of a slightly pale red tint. The thalamus opticus and corpus striatum on each side were also somewhat hardened, but less so than the white matter. The ventricles contained only a small quantity of limpid serosity. The cerebral protuberance appeared to have undergone a slight increase in consistence, but this was not very apparent. The spinal chord was small and slender, as if atrophied. The membranes were healthy; and the vertebral canal presented no trace of tumour or other appreciable

lesion. The medulla, on being examined very carefully throughout its whole extent, was found to possess everywhere its natural colour and consistence; no trace of ramollissement or of hæmorrhage could be discovered. The other organs presented no remarkable appearance. [*Gazette Médicale de Paris*, 9 February, 1850.]

CRYPTOGAMIC VEGETATIONS IN THE INTERIOR OF THE HAIR IN FAVUS.

Dr. C. Wedl has published an account of his having discovered Cryptogamic Vegetations in the interior of the hair in Favus. After briefly referring to the observations of Gruby, Günsburg, Malmsten, and Lebert, he says:—

I directed my attention to the interior of the hair, making use of a concentrated alkaline solution to render it more transparent. This solution acts on some parts of the hair in a very remarkable manner; in different strata of it, there appear small elongated gas-bubbles, with a sharp dark outline, giving the appearance of an interrupted canal; sometimes also larger gas-bubbles are seen lying at the outside of the hair. The latter gradually becomes more transparent; and, in the situation of the dark broken canals, there appear filaments formed of elongated quadrangular or roundish molecules; these extend through a certain portion of the hair, here and there giving off branches, and are to be found in all layers of the hair. These filaments consist of granules arranged like beads. Their transverse diameter varies; their length is also very variable; here and there, only a few granules are linked together, while in other parts there are several dozens of them. The branches which spring from them are sparse, generally very short, and always single; the direction of these filaments is in the longitudinal axis of the hair. I have never seen them hanging from the outside, unless I had torn the sheath of hair, or the whole hair, by rubbing it to and fro on the glass, after it had been acted on for an hour by strong liquor potassæ. On the surface, one often indeed sometimes sees regularly arranged filaments surrounding the hair; but the most beautiful and conspicuous are these transverse granular filaments, at the place of exit of the hair from the epidermis. In the latter, which often remains hanging when the hair is torn out, are usually some pretty numerous groups of granules (sporidia), and here and there a granular filament comes into view, which passes transversely across the hair, or even seems to end in it.

I could not ascertain with precision the entrance of the filaments of the dependent epidermis into the hair; but I have no doubt that such is the case, because these granular filaments are most abundant at the above-mentioned point of exit; moreover, they decrease as they proceed upwards, and disappear at the end of two or three lines; they extend some way down, but not in such numbers, and do not seem to reach the bulb. The point of concentration of these filaments is, then, the place where the hair emerges from the epidermis; it is here surrounded with the vegetations of favus. In order to establish the fact, that the filaments in the hair are really fungous growths, it is necessary to examine fungi, but especially those of favus, under the action of strong liquor potassæ. There also is an extrication of gas under the microscope; the sporidia and thallus fibres are not dissolved, but are rendered more conspicuous, while the epidermis cells become faint. Dr. Hebra perceived the development of a fœtid gas in his researches, when fungi were exposed longer than usual to the action of liquor potassæ.

With regard to the frequency of this appearance, it is to be observed, that in most cases of favus, it is possible to find these fungi in the hair, especially when light. When it is dark-coloured, it requires to be macerated about half an hour in strong liquor potassæ; but, in light hairs, the sporidia can be seen in from five to ten minutes.

This remarkable circumstance—the growth of fungi in the interior of the hair in favus—gives an explanation of its well-known dryness and brittle-

ness, for the nutrition of the hair must be interfered with by the parasitic structures. Moreover, these observations throw light on the long known rule in dermatology, that, in order to cure favus effectually, the hair must be removed. [*Zeitschrift der K. K. Gesellschaft der Aerzte zu Vien.* Fünfter Jahrjang 1849, p. 643.]

MUCOUS PNEUMONIA, OR SUFFOCATIVE CATARRH, OF VERY YOUNG CHILDREN.

Every one who has been in the habit of carefully observing and reflecting upon the phenomena of disease in early infancy, must have been struck with this remarkable fact—that extensive pneumonia is, comparatively speaking, often, not only *easily*, but *speedily* cured; while that which is circumscribed and apparently less formidable, has, more commonly, a fatal issue. We think the following remarks, by M. Duclos, are interesting, and important in their bearings upon this subject. We concur generally in his treatment, though we consider it defective, in not pointing out the danger of fatal depression, if the emetics be pushed too far, especially when they do not cause vomiting: in the omission of an account of the precautions required in such cases; and in the absence of rules for the management of the skin. Our abstract is chiefly taken from the *Revue Médico-Chirurgicale de Paris*, for January 1850, p. 38.

Memoir of M. Duclos. M. Duclos has seen, at the Hospital for Children, cases of double pneumonia, in which the whole pulmonary parenchyma was involved, and in some spots presenting hepatization, yield after from nine to twelve days—the mean duration of pneumonia in early infancy: whereas, others in which the affection was more limited, without reaching the stage of hepatization, have, after four or five days, terminated in death. He has found that these two classes of cases differ essentially:

1st. In the mode of production. 2nd. In the symptoms. 3rd. In the anatomical characters.

M. Duclos designates the rapidly fatal form of the disease Mucous Pneumonia; being what others have described under the names of *Suffocative Catarrh* and *Capillary Bronchitis.*

1. *Mode of Production.* Vigorous, strong children with *fat* chests, are those liable to Mucous Pneumonia. Most commonly, it begins as a simple bronchitic attack. The chest, as ought to be the case, is probably auscultated at brief intervals during the day, without any other symptom being indicated; when at last, upon some one occasion, signs of inflammation of the substance of the lungs are discovered. Mucous Pneumonia may be considered as the sequel or extension of a catarrhal phlegmasia. It is important to bear in mind, that it occurs most frequently during the first dentition. The ordinary pneumonia is more generally a distinct disease.

2. *The Symptoms* also present essential differences. In the ordinary pneumonia of very young children, a subcrepitant râle is heard when lobular inflammation exists, and a *souffle* is also heard in like circumstances. There exist likewise oppression, agitation of the alæ nasi, and a sulcus at the base of the chest corresponding to the insertion of the diaphragm; a sulcus so very characteristic as a symptom, that M. Trousseau has called it the *sillon peri-pneumonique.* Fever attends the above-named symptoms. The latter symptoms are to be met with in Mucous Pneumonia, but those furnished by auscultation are different; there is no *souffle* to be heard, because the pneumonia is not lobular. In Mucous Pneumonia, the bronchial secretion is extremely abundant; the mucous râle masks the sub-crepitant râle; and the mucous râle is so loud that it may be heard by approximating the ear to the chest, without actually applying it.

3. *The Anatomical Characters* of bronchitis and lobular pneumonia are both found in the Mucous Pneumonia; and it is on this account that M. Duclos

has adopted that name for the disease. On slicing the lung with the scalpel, a marbled aspect is presented; some lobules are decolorized, or of their natural hue, while others appear as if deeply stained with the lees of wine, and which have evidently been the seat of inflammation. If the last-mentioned lobules be carefully isolated and placed in water, they sink to the bottom; but the others, which have not been the seat of inflammation, float on the surface when similarly treated, and even after an attempt has been made to press out the air with the fingers. The bronchial ramifications are choked with mucus.

Prognosis and Treatment. Physicians form very often a favourable prognosis in Mucous Pneumonia, which, to their dismay, is abruptly disproved by the death of the patient. It ought always to be looked upon as a most formidable complication of pulmonary disease. With regard to treatment, M. Duclos shows that it must be different from that of ordinary pneumonia; *first*, on account of the rapid progress of the malady, from the onset of the inflammatory symptoms; *second*, from the mechanical obstacle to respiration, caused by the choking of the air-passages by mucus. Emetics are essential; and a syrup of tartar emetic and ipecacuanha is to be given every ten minutes, till the infant have vomited four or five times. The medicine is then suspended, and it is again exhibited in the same way, morning and evening, till the same effect be obtained. It is not so easy to produce vomiting after the first time, so that larger doses, and the drinking of tepid water as an adjuvant, are required. During the intervals between the excitement of vomiting, an antimonial mixture, with a small quantity of opiate syrup, is given; but this is a secondary and not an essential part of the treatment. In the same category he places counter-irritation of the skin, whether by embrocations of croton oil, etc., or by blisters, which, when he does employ, he puts on the legs, in preference to the anterior or posterior part of the chest. Blisters early applied to the legs, and made to discharge freely, M. Duclos considers very useful. His treatment may be thus summed up:

1. Emetic treatment as described—Essential.
2. Use of antimonials in the intervals. } Useful.
3. Counter-irritants, especially blisters to the legs. }

CHLOROFORM ADMINISTERED WITH ADVANTAGE IN LEAD COLIC: ANODYNES IN LEAD COLIC.

Dr. Blanchet, in the *Journal de Chimie Médicale*, recommends this practice. He details two successful cases. Believing that the best method of treatment is, in the first instance, to give energetic anodynes and antispasmodics, we are prepared, *à priori*, to approve of this recommendation of Chloroform in Lead Colic; but the form of administration, which we would decidedly prefer, is Smith's mixture (with camphor), both from its convenience, and from the therapeutic utility, in such cases, of both ingredients. We have not had an opportunity of giving this mixture in lead colic, but we have largely used it in analogous cases; and when an anodyne and antispasmodic is required, we know of no better prescription. Notices on this subject will be found in our volume for 1849, pp. 102 and 842. Belladonna, applied externally, we have several times used with success in cases of Lead Colic. The patient obtains speedy relief; and the cure is completed by one or two doses of castor oil, or a mercurial purge, with or without camphor and henbane, as the case may require. Opium may, in some subjects, be found more useful than any other medicine. The important principle to bear in mind is this—that the spasm of the intestines must be first subdued; and that till this be accomplished, purgatives only aggravate the severity of the symptoms.

SURGERY.

ON LITHOTOMY AND LITHOTRITY: BEING AN ACCOUNT OF THE EXPERIENCE OF MM. PAMARD, SENIOR AND JUNIOR, FROM 1792 TO 1849, EMBRACING SEVENTY-TWO CASES.

The following is the substance of a paper (*Revue Médico-Chirurgicale*, May 1849), by M. PAMARD, Surgeon to the Hospitals of Avignon, in which he describes all the cases of Lithotomy and Lithotrity which had come under himself and his father. Documents of this description are as valuable as they are rare.

I am one of those who attach great value to authentic and well-compiled medical and surgical statistics. In furnishing such contributions to science, country practitioners possess an advantage. Their practice is daily open to the observation of their brethren, who live, like themselves, in the midst of the patients, and exercise such a check, as to render it very difficult for errors, either voluntary or involuntary, to occur. I proceed to give the operations of my father; giving first the cases upon whom Lithotomy was performed more than once; and then those who died within two months after the operation. The other cases are arranged according to their date. From the 5th May 1792, to the 19th May 1818, my father performed Lithotomy in sixty cases, of which he lost five: but the sixty operations only represent fifty-five patients, because one was cut thrice, and two were cut twice.

CASE 1. V. Duclos, of Pont-Saint-Esprit (Gard), aged 68, was cut in the year VII (A.D. 1797), by an operator of Grenoble. On the 30 fruct. year VIII (1798), my father operated for the first time. A stone again formed; and was removed on the 5 vend. of the year X (1800), and a third stone was removed on the 1st vend. of the year XII (1802). The patient survived a long time after the third operation, without another stone forming.

CASE 2. A. J. Jouve, aged 48, of Gordes (Vaucluse), was cut for stone, on the 13 floréal, year VI (1796). A fistula formed, which probably was the cause of the production of a new calculus; for which a second successful operation was performed on the 15 frimaire, year VII (1797).

CASE 3. J. Ricard, of Cavaillon (Vaucluse), aged 15, was cut the first time on the 8 frimaire, year VIII (1798); and the second operation, which was successful, was not performed till the 27th September 1811.

CASE 4. J. Lombard, of Avignon, was cut for stone on the 23 brumaire, year VII (1797), having had the same operation performed when 4 years old by my grandfather. The stone never formed after the second operation.

CASE 5. J. Veyren, aged 19, of Villeneuve (Gard), had Lithotomy performed on the 4 floréal, year VIII (1798). My father conjectured that the first operator had left fragments of the calculus in the bladder. The operation was repeated on the 3 frimaire, year IX (1799); and the patient recovered.

CASE 6. T. Morgan, of Caumont (Vaucluse), a female, aged 8 years, had suffered from her birth. Lithotomy was performed on the 30 vendémiaire, year III (1793). The operation was difficult, and the bladder was torn at its fundus. The stone was very large, considering the age of the patient, weighing 105 grammes (about 27 drachms apoth. weight). The child died on the fifth day after the operation.

CASE 7. N., a man, aged 72, of Avignon, was cut on the 20th August 1792. He had suffered for 20 years, and was only driven by pain to submit to the operation. The pubic artery was probably involved: the stone was very large and jagged. After a succession of hæmorrhagic attacks, the patient died on the 26th September.

CASE 8. J. O., a boy, aged 10, of Crillon (Vaucluse), was cut for stone on the 8 floréal, year VI (1796). Owing to the size of the stone, the operation was long and painful. It broke under the pressure of the forceps, which

necessitated their repeated introduction. Death, from inflammation of the bladder, ensued three days after the operation.

Case 9. F. C., a lad, aged 19, of Malaucène (Vaucluse), was cut on the 23 brumaire, year VII (1797). The operation was easily and rapidly performed; but the patient died from phlebitis on the eighth day.

Case 10. M. V., a man, aged 25, was cut on the 16 floréal, year VIII; and though there was no complication in the operation, it was followed by death from phlebitis on the 27th of the same month.

Case 11. P., a boy, had a small stone removed by Lithotomy, on the 5th May 1792. Rapid recovery.

Case 12. V., male, aged 17; cut 19th April 1797. Rapid recovery.

Case 13. M., aged 47, a man, was cut 18th May 1793. Rapid recovery.

Case 14. Fabre, aged 8 years, cut 9th May 1793. Recovery.

Case 15. F., a boy, aged 4 years, cut 27th September 1793. Rapid recovery.

Case 16. R., a boy, aged 7 years, cut 21st September 1793. Recovery.

Case 17. M., a boy, aged 10 years, cut 7th October 1793. Hæmorrhage occurred, requiring a canula, armed with agaric, to be introduced; but, nevertheless, a rapid recovery ensued.

Case 18. P., a boy, aged 3 years and 8 months, cut 9th October 1793: stone small; recovery rapid.

Case 19. T., a boy, aged 17, was cut 4th January 1794. Recovery.

Case 20. T., a boy, aged 12, cut 2nd May 1795. Rapid recovery.

Case 21. F., a young lad, cut 2nd May 1795. Rapid recovery.

Case 22. R., a boy, aged 7, cut 2nd June 1795. The stone was fractured; the operation was protracted, and recovery took two months.

Case 23. R., a man aged 80, cut 27th May 1796. He had hæmorrhage, requiring the introduction of a canula. Recovery.

Case 24. G., a man, aged 24, cut on the 3 brumaire, year VII (1797). Recovery.

Case 25. R., a boy, aged 5, cut 16 frimaire, year VII (1797). The stone was large; and the seizing and extracting of it were difficult. Recovery.

Case 26. A., a boy, aged 5½ years, cut 16 floréal, year VII (1797). Recovery.

Case 27. R., a man, aged 75; enormous stone extracted by Lithotomy on 10 floréal, year VIII (1798). Recovery.

Case 28. L., a man, aged 47, cut 3 fructidor, year VIII; the stone was enormous. Recovery took place, but with a recto-vesical fistula, evidently originating in a wound of the rectum.

Case 29. A., a boy, aged 9 years, cut 5 floréal, year VIII (1798). Speedy recovery.

Case 30. R., a boy, aged 9 years, cut 6 frimaire, year X (1800); two small calculi. Speedy recovery.

Case 31. G., a boy, aged 5 years, cut 20 floréal, year X (1800). Recovery.

Case 32. B., a boy, aged 4 years, cut 24 floréal, year XI (1801); mulberry calculus. Rapid recovery.

Case 33. B., a boy, aged 8, cut 9 thermidor, year XII (1802). Recovery.

Case 34. D., a young lad, cut 2 messidor, year XII (1802). Recovery.

Case 35. G., a boy, aged 4½, cut 27 brumaire, year XII (1802). Recovery.

Case 36. C., aged 27: cut 3 floréal, year XIII (1803) The calculus was large and friable; the operation was prolonged. Recovery.

Case 37. D., a boy aged 13: cut 21 floréal, year XIII (1803). Recovery.

Case 38. D., a doctor in surgery, aged 65; he had suffered for twenty years, but had never allowed himself to be sounded. Pain, at last, overcame his reluctance: and he was cut for stone on 15th March 1806. The stone was very large; but nothing untoward occurred, and recovery was rapid.

Case 39. S., a man aged 22: cut 3rd June 1806. Recovery.

Case 40. C., a boy aged 11: cut 13th September 1806. Speedy recovery.

Case 41. R., a man, aged 31 : cut 6th December 1806. From the large size of the stone, force had to be used in the extraction ; nothing untoward occurred. Recovery.

Case 42. T., a boy, aged 8 : cut 24th October 1806. The stone was large and friable ; the child had suffered from his birth ; the operation was prolonged : recovery took place, but there was a urinary fistula.

Case 43. Servant, the curate of Tulette (Drôme), aged 68 years, was cut on the 9th May 1808 ; he was a lusty subject, and the operation was laborious ; but the success of the operation was complete—a fact worth notice, as Roux, the famous operator of the Hôtel Dieu, states that every ecclesiastic whom he has cut for stone, has died.

Case 44. S., aged 56 years : cut, 2nd of September 1810 : large stone. Recovery.

Case 45. T., a man aged 74 : cut 3rd of October 1810. The stone was very large, and weighed 110 grammes (about 28¼ drachms apoth. weight). In spite of the age of the patient, and the force required in extraction, recovery was quick and complete.

Case 46. F., a man aged 50 : cut 15th October, 1810. Recovery.

Case 47. C., a man aged 22 : cut 4th October, 1810. Stone large. Recovery.

Case 48. Madame Valentin, of Avignon, aged 25 : cut 7th October, 1811. The stone was very large, and its extraction was difficult. The incision, which was low, and on the left, involved the vagina. Recovery took place, but she had incontinence of urine.

Case 49. Madame Cambon, aged 21 : operated on in the same way as Madame V., on 10th November, 1812. She recovered, and had no remaining inconvenience.

Case 50. P., a boy aged 12 : cut 24th September, 1812. Recovery.

Case 51. Melin, aged 3½ : Lithotomy 17th May, 1814 : small calculus. Speedy recovery.

Case 52. Reynaud, aged 64 : large calculus extracted 12th May, 1817 : considerable hæmorrhage. Recovery.

Case 53. Joussaud : cut 16th June, 1817. Recovery.

Case 54. Jeaume ; suffered from birth : cut 14th June, 1817. From the number of calculi, the operation was laborious. Recovery in ten weeks.

Case 55. M., a woman aged 58 : cut 10th June, 1817. Very large stone. Complete recovery.

Case 56. R., a man aged 17 : cut 19th May, 1818. Success complete.

I now proceed to detail my own experience ; *first*, in Lithotomy ; *second*, in Lithotomy preceded by attempts at Lithotrity ; and, *lastly*, in cases in which Lithotrity alone was performed.

I. Lithotomy. Case 1. M. Baldy, of Nîmes, aged 15, had, when six years old, been cut for stone, by M. Pleindoux. Till within two years, he had retained a fistula in perinæo : operated on, 2nd March 1832. Dismissed cured on the 8th April.

Case 2. M. Martin, of Avignon, aged 20, had suffered from infancy ; but neither her pain, nor the entreaties of her relatives, could induce her to submit to an operation. The operation was ultimately performed on the 18th of January 1837, under the disadvantageous circumstances of a presentiment on the part of the patient that she would not recover. Lithotrity appearing inadmissible from the large size of the stone, and the very irritable state of the urethra. The stone weighed 77 grammes. For the first days, the patient appeared to be doing well ; but, on the fourth, rigors and fever set in, and she died upon the 24th of January. On dissection, I found the mucous tunic of the bladder red and soft ; there was pus in the iliac veins. The abdomen was distended with gas, and the peritonæum dotted with red spots.

Case 3. B., of Monteux (Vaucluse), aged 9 years, was operated upon, at his father's house, on the 21st of September 1836. The stone weighed ten grammes. Recovery was complete fourteen days after the operation ; and the patient, who is now a military officer, has never had any return of the affection.

Case 4. M. C., aged 72, of Entrechaux (Vaucluse), a healthy old man, after suffering for four years, was operated upon on the 11th May 1840. I removed three calculi, smooth and presenting facets formed by friction against each other, and weighing, collectively, 80 grammes. The cure was complete on the 28th June.

Case 5. L., aged 21, of Fresney (Mayenne), who had suffered from his earliest years, was operated upon in hospital on the 10th of January 1841. The stone was bulky and indented, and weighed 45 grammes ; it broke under the forceps, but was all removed : and the patient was dismissed cured on the 5th of February.

Case 6. M., a soldier, aged 27, of Bourret (Tarn-et-Garonne), was operated upon on the 18th of January 1842, in hospital. The stone was hard, voluminous, and brown in colour ; weighed 60 grammes, and was extracted entire. The wound shewed an indisposition to heal, which rendered it necessary to introduce deeply the solid nitrate of silver. On the 17th of May, being four months after the operation, he went out completely cured.

Case 7. M., aged 15, of Villeneuve (Gard), a thin and irritable youth, had suffered from his earliest recollection ; but his parents could never persuade him to allow himself to be sounded, till I was permitted to do this, and discovered a hard calculus with unequal surface. The extremely sensitive condition of the urethra rendered Lithotrity inadmissible ; and I performed Lithotomy on the 20th of February 1844. The stone was very large, and very light, not weighing more than 8 grammes. On the 22nd of March the patient left the hospital, perfectly cured.

Case 8. N., a man aged 27, of Labastal (Hautes-Pyrénées), had endured a miserable existence for a long time previous to his entering the hospital, on the 28th of February, 1845. I cut him on the 2nd of March. The case presented a circumstance worthy of being mentioned. There was contraction of the bladder to such an extent, as to impede the separation of the blades of the forceps. The stone, however, was seized and extracted ; it weighed 32 grammes. Its appearance might have suggested the idea, that it was a substance moulded in the bladder of a child. On its upper part there was a plane surface ; but, upon digital exploration, I satisfied myself that there was not a second stone. The patient left the hospital cured, on the 11th of April.

Case 9. G., aged 4½ years, of Gravéson (Bouches-du-Rhône), had suffered from the time of his lactation. I performed Lithotomy on the 22nd of July, 1847, as, in young children, I infinitely prefer this operation to Lithotrity. He was placed under the full anæsthetic influence of ether. I used polypi forceps, being an instrument which I prefer in children. I removed an oval calculus, of the size of a large bean ; after which, I introduced my finger (as I always do in children), and discovered a second calculus, of the size of a pea, which was easily extracted by means of the index finger, without the aid of any instrument. The two stones, collectively, weighed 3 grammes. On examining the second calculus with care, it was found to be covered with facets, which led me to conclude that there were a group of small calculi ; but, on again introducing the index finger, I found the bladder completely unembarrassed. The only explanation which can be given of these facets, is to suppose that friction took place between the two calculi. The issue of this case was fortunate. At the end of a month, the child was well ; and, though thin, he was in good health. I have seen him this winter, and can testify that he is a superb child, and in the enjoyment of perfect health.

Lithotomy preceded by attempts at Lithotrity. Case 10. A. B., of Avignon, aged 8 years, was brought into hospital on the 27th March,

1845. I detected a small calculus in the bladder; and, yielding to the wishes of the parents, though in opposition to my own opinion, consented to perform Lithotrity. Having dilated the urethra, I introduced Charrière's modification of Heurteloup's lithotrite. I seized the calculus, and broke off a pretty large fragment, which was voided into the bath in which I placed the patient immediately after operating. The operation was quickly performed; but the restlessness of the patient induced such severe pain, that I thought it my duty at once to propose lithotomy, as offering less inconvenience than Lithotrity. I performed the operation on the 18th of April, and it was followed by quick recovery.

If we may judge from the complaints of the patient, he would seem to have suffered less from the operation of Lithotomy than from Lithotrity, although the calculus had been readily seized and broken. The calculus extracted from the bladder, weighed 5 grammes; and the piece broken off by the lithotrite, one gramme and 50 centigrammes.

Case 11. Mademoiselle P. M, aged 17 years, of Angles (Gard), had been the subject of calculus for some years, the existence of which she concealed for a considerable time. Her family attendant having discovered its presence, advised Lithotrity; and she was placed under my care. On the 10th of May 1847, I operated on her at Angles, in the presence of Drs. Salomon, senior and junior, Dr. Touzet, and others. The patient having been placed under the influence of ether, I easily introduced a No. 3 Lithotrite, and immediately found a large calculus. The instrument was opened in the bladder with some difficulty; the calculus was broken at three attempts. The patient experienced no pain whatever; and, when she awoke, said she thought she was at church with her father. She was placed in a bath immediately after the operation, where she voided some fragments of the calculus: she also voided some fragments, and a large quantity of detritus, during the night.

The operation was twice repeated, at intervals of a few days; but we found that the calculus still remained large. The patient experienced pretty severe pain in passing the larger fragments; and on the last two occasions of operating, she suffered severe pain, and was very restless: she refused to be etherized. Considering it imprudent to continue Lithotrity, we proposed that Lithotomy should be performed. This was accordingly done on the 25th of June, 1847. We adopted the method improperly attributed to Dubois, but which, according to Parè, belongs to Laurent Collot, a surgeon of the sixteenth century. The incision was made directly upwards, with the lithotome of Friar Côme. As the calculus had been broken in several pieces, we had to introduce the forceps several times. The largest fragment measured 4 centimètres in its greatest diameter. The fragments which had been passed before this operation, weighed 7 grammes; and those which we extracted, amounted to 26 grammes. The result of the operation was most fortunate: on the twentieth day, the patient was completely cured, and was able to visit me at Avignon.

This result must be allowed to be infinitely preferable to that which must have been produced by Lithotrity. This proceeding would have required, perhaps, fifteen or twenty sittings, each nearly as painful as Lithotomy, and have exposed the patient to the risk of fatal cystitis.

We now come to a case in which Lithotrity was impossible, and Lithotomy was the only resource.

Case 12. Mademoiselle M. M., aged 30 years, of Malaucène (Vaucluse), had, a month previously, introduced a pen-case into the urethra, which escaped, and fell into the bladder. She came to me, and, with much hesitation, confessed what she had done. I detected the presence of the foreign body by means of a catheter, and endeavoured, but in vain, to extract it with Heurteloup's lithotrite. It was easily seized; but, on attempting to extract it, resistance was encountered, and the patient was put to great pain. It was evident that the foreign body lay transversely, and that it was probably impossible to make it change its direction. We proposed Lithotomy, which

was immediately accepted. On the 19th June 1847, having perfectly etherized the patient, I operated with a lithotome, making the incision directly upwards. I introduced the index finger of my left hand, and discovered that the body was actually placed transversely: I caused it to assume a longitudinal direction, and introduced a pair of polypus forceps along the finger, by the aid of which I extracted the substance without difficulty. It was almost entirely covered with an incrustation of phosphate of lime; there was a depression at the point where it had been seized by the lithotrite: it was eight centimètres in length, and weighed three grammes.

The rapid success of this operation could not have been surpassed. Eight days after the operation, the patient was able to return home, perfectly cured.

It is somewhat remarkable, that, a few days after, I read in the *Bulletin de l'Académie* (15 July, 1847), an account of a perfectly similar case to that which I have just related, occurring in a male. As in my patient, Lithotrity was impracticable, and Lithotomy was followed by cure.

Lithotrity. Case 13. M. P., of Avignon, aged 68 years, had long suffered from calculus; but he would not consent to Lithotomy. The pain, however, becoming more severe, the general health being impaired, and the urine depositing a glairy fœtid matter, he was compelled to decide on an operation, and chose Lithotrity. On the 25th January 1832, after having previously calmed the patient with baths and refreshing drinks, we proceeded to the operation, using M. Civiale's instruments, which were then generally employed. Although I had several times performed Lithotrity on the dead body, and had demonstrated it to the pupils at the hospital, this was the first occasion of my operating on the living subject.

The instruments were introduced easily enough; but there was difficulty in seizing the stone, from its large size, and from the contracted state and excessive sensibility of the bladder. At last, however, the stone was seized, and perforated in several points, by slightly relaxing the hold and rotating it. This sitting lasted eight minutes. A pretty large quantity of detritus was passed on the same day: on the next, the patient felt tolerably well; but, two days after, there was tenderness and tension in the hypogastrium, with difficult passage of urine. This was ascertained, by means of a sound, not to arise from the retention of a fragment of the calculus in the urethra. A large number of leeches were applied; and baths, lavements, with calmative draughts and applications, were prescribed; but nothing could arrest the progress of the inflammation of the bladder. The patient went on getting worse, and died on the 26th February, a month after the operation. Although the cystitis only appeared on the third day, and death did not take place for a month, we think that no one can doubt that the Lithotrity was the cause of death. At the autopsy, we found the calculus broken into numerous fragments. It weighed altogether 52 grammes, and the nucleus bore the mark of the perforator. The mucous membrane of the bladder was red, softened, and covered with a layer of purulent mucus, having the appearance of a false membrane. The peritoneum presented some red points of inflammation. The abdomen was distended with gas: this had distressed the patient during the whole course of his illness.

Case 14. M. R., of Orange (Vaucluse), had Lithotrity performed by M. A. Jourkoski, on the 3rd of October 1841. I was not present at the operation, but only know that percussion instruments were used, and that there is no reason to doubt that it was performed skilfully. On the 5th of October, two days after the operation, I was called to the patient, who was in a state of ardent fever, with burning skin, tension, and pain in the abdomen, and difficulty in micturition. By aid of the catheter, I recognized the presence of a calculus, and drew off a large quantity of urine. The patient felt great pain in the bladder, and earnestly requested me to relieve him by an operation. I confined my treatment, however, to calmatives and antiphlogistics, but without any hope of a favourable result. In this I was not mistaken, for the patient died on the following day. An autopsy was not made. In this

case, death was so evidently and rapidly the result of a single sitting of Lithotrity, that we do not think that the most ardent admirers of this operation can mistake it. We will now relate some successful cases.

Case 15. E. B., aged 16, of Tarascon (Bouches-du-Rhône), had been suffering for about a year, and was supposed to have a calculus. He came to Avignon to consult me ; I recognized the presence of a small stone, and proposed Lithotrity, to which he consented. On the 5th of March 1832, we operated with Civiale's instrument. The instrument was easily introduced, and the calculus was readily seized and broken up, being small and friable. The patient experienced severe pain during the search for the calculus, but afterwards did not complain. A pretty large quantity of detritus was passed in the warm bath, and a larger quantity during the night ; some of the fragments were rather large, and gave some pain. The whole of the detritus weighed three grammes. The next morning, the patient had no pain ; and, though we sounded him on several occasions, we could discover nothing in the bladder. He was evidently perfectly cured. If Lithotrity were always attended with such results, it would be one of the most splendid operations.

Case 16. M. J. A., aged 82, of Avignon, presents us with an interesting case. He was a timid man, who had suffered for a number of years, but had never been able to determine to be operated on, not even to be sounded. At last he consented ; and I recognized the presence of several apparently hard and resistant calculi, which gave a very distinct sound on being struck. Lithotrity seemed the only practicable operation ; it was accordingly performed for the first time on the 25th of February 1833, with M. Heurteloup's instruments. The canal was very large ; the instrument was easily introduced, and I soon seized a large calculus, about the size of an egg, which offered some resistance to the action of the lithotrite, but which I succeeded in breaking down. The patient groaned a good deal, but did not seem to suffer much ; he appeared to have more fear than pain. He immediately voided a considerable quantity of detritus. Four days after, the operation was repeated with the same results ; and we were obliged to have recourse to it fourteen times. He was under treatment up to the end of October ; and there were several circumstances which gave me anxiety. On several occasions, the operation was followed by an attack of violent fever ; and the patient each time became more impatient of the operation, and appeared to suffer more. At last I had the happiness of telling him, that I could find nothing in his bladder. The disappearance of pain confirmed my diagnosis ; and, in spite of his advanced age, M. A. was restored to health.

It cannot be denied, that, in spite of the pain and of the difficulties attending the operation, the success was due to Lithotrity. The detritus, as far as they could be collected, weighed 20 grammes ; but it is probable that the patient lost a considerable quantity.

Case 17. M. B , aged 22, of Bollênes (Vaucluse), had suffered for several years from a single and rather small calculus ; his general health was good. On the 9th of May 1840, we performed Lithotrity with M. Heurteloup's instrument, and repeated the operation thrice subsequently. The patient returned home, perfectly cured, on the 9th of June. The detritus of the calculus weighed 6 grammes.

Case 18. M. A. A., aged 22, of Cébagola, (Corsica) presents us with an analogous case. First sitting, 21st of March 1842. Calculus of moderate size, but hard : operation performed with percussion instruments : cured on the 19th of April, after five sittings. We collected 15 grammes of detritus.

Case 19. M. P. R., aged 17, of Avignon, was as fortunate as the two preceding patients. On the 15th February 1843, we operated for the first time ; but the narrowness of the canal caused considerable resistance and pain. The calculus was small and friable ; in fact, the smallest I ever met with. The collected fragments weighed 10 grammes. The operation was repeated four times, at intervals of a few days : on each occasion, fever supervened. The patient was cured in the space of a month.

Case 20. M. M., aged 25, of Borey (Haute-Saône), had suffered several years from stone. He told me that M. Pétrequin had discovered the existence of one, at Lyons; but he had not been able to determine to submit to an operation. Having, however, seen a patient at Avignon, on whom I had operated, he requested me to perform Lithotrity. To this I consented; and, on the 14th January 1844, operated for the first time. The stone was large; the graduated scale on the instrument indicated 35 millimètres: it was easily broken down, but offered some resistance, which increased towards the centre. The patient experienced some pain, from the retention of one of the fragments in his urethra; and I was obliged to remove it with a three-branched forceps. The operation was repeated six days after, and in all, nine times. The patient was under treatment during two months, and was found to be cured on the 11th March. The calculus weighed 30 grammes: it was formed of two layers; the external soft, friable, consisting of phosphate of lime; the internal hard, brittle, composed of uric acid.

Remarks. When we take into consideration the duration of the treatment, the number of sittings which were necessary, the necessity of using Hunter's forceps to search for fragments—we may ask, whether Lithotrity possesses any marked advantages over Lithotomy. It will be seen that, out of sixty patients operated on by my father, there were only five deaths, or one in twelve. I have arrived at exactly the same result: of nine patients on whom Lithotomy was primarily performed, one died; and of three, in whom Lithotomy was preceded by attempts at Lithotrity, none died; which gives one death in twelve cases of Lithotomy.

In Lithotrity, I have been less fortunate; for in eight operations, seven by myself, and one by another operator, two patients died. This gives a proportion of one in four. I have three times found Lithotrity useless, or attended with such inconvenience that I was obliged to have recourse to Lithotomy. I know that it will be said, that I do not praise Lithotrity, because I do not know how to perform it. This reproach, however, has been addressed to my illustrious masters, MM. Roux and Velpeau.

My father had five relapses among fifty-six cases; and in the case of M. Baldy, of Nîmes, I found a calculus in the tissue of the perineum. In 72 operations, then, there were six cases of relapse. I would desire much to know, whether, in an equal number of cases of Lithotrity, there would not be a greater proportion of relapses.

OPERATION FOR THE EXTRACTION OF A WOMAN'S DOUBLE HAIR-PIN FROM THE MALE URETHRA.

Dr. Soulé, Surgeon to St. Andrew's Hospital, in Bourdeaux, has published the following case in the *Journal de Médicine* of that city. A man, aged 23, came to Dr. Soulé, stating that his mistress had, while he slept, introduced into his urethra, one of those black forceps-shaped pins which women use for arranging their hair; but he afterwards owned that he had himself committed the folly. The blunt curve had been pushed inwards; and the two sharp points were scarcely two centimètres from the orifice. Attempts at extraction by the natural passage having failed, the penis was strongly bent, so as to cause the points of the pin to traverse the inferior wall of the urethra. The curved portion uniting the two stems was then straightened, and one of the stems was cut off close to the penis, when the extraction was completed with much difficulty. The penis was enveloped in compresses wetted with cold water; and in two days the patient left the hospital, presenting no trace of what had happened, except two ecchymotic spots, corresponding to the two punctures made by the points of the pin. The procedure of Dr. Soulé is similar to that recommended in 1847, by Dr. Boinet, in the *Journal des Connaissances Medico-Chirurgicales*, to which work we must refer for farther details.

REPORTS OF SOCIETIES.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY,

Tuesday, January 22, 1850.

Dr. Addison, President, in the Chair.

Chemical Researches on the Nature and Cause of Cholera. By Robert Dundas Thomson, M.D., Glasgow. Communicated by Sir B. C. Brodie, Bart. In the first part of the paper, the author detailed the results of chemical analyses of the blood, urine, and intestinal discharges, in the cold, or "lymphatic," stage of cholera; and in the "biliary," or febrile stage. The main results arrived at were—1. That in the cold stage of the disease, the specific gravity of the blood, and of the serum separated from the clot, is increased; that the proportion of water is less than in health, by at least nine per cent., and in some cases by as much as seventeen per cent.; that both the organic and the inorganic components of the blood are proportionally increased in amount; but that the increase of the insoluble salts is much greater than that of the soluble. 2. That the intestinal discharges, in the cold stage, when of the true "rice-water" character, resemble closely, in their chemical composition, the fluids of hydrocele and hydrocephalus; that their flocculi are formed of epithelial scales, and the watery part of water, containing a small portion of organic matter (albumen) and salts (chloride of sodium, carbonate of soda, earthy phosphate, alkaline sulphate, and some lime). 3. That the small quantity of urine sometimes found in the bladder, in this stage, presented no apparent aberration from an ordinary standard. 4. That in the biliary, or febrile stage of cholera, the blood soon regains its normal proportion of water, or even an excess of it; and that the other constituents resume their natural relation to each other. 5. That the urine, in the biliary stage, in several cases contained albumen, but presented scarcely any other deviation from the urine of health, except in the amount of urea, which at first was deficient.

In the second part of the paper, the author described some experiments, instituted by him, with the view of determining whether any poison could be detected in the atmosphere. In one series of experiments, it was ascertained that no solid matter existed in the air; but ammonia was obtained from it in the proportion of 0·319 grains of caustic ammonia, or 0·731 grains of carbonate of ammonia to 1,000 pounds of air. By another series of experiments, it was determined that no carbon or hydrogen existed in the atmosphere, except in the states of carbonic acid and water; while carbonic acid was obtained in the proportion of one volume to 6,650 volumes of air.

The author inferred, that *the cause of cholera is not a specific tangible poison, introduced into the body from without, but rather a vicarious transference of the cutaneous excretion to the intestinal mucous membrane, dependent partly on atmospheric influence, and partly on a predisposing state of the system in those who are affected with the disease.*

Dr. Snow addressed the society upon the peculiar views which he holds regarding the communication of cholera, and which have already been laid before our readers. [Vide London Journal of Medicine for 1849, pp. 1077, 1083, and 1123.]

Dr. Copland thought the experiments were too few in number to justify any definite conclusions. He feared that the emissaries of the Board of Health had been employed to support preconceived opinions, and not to elicit truth. Many misstatements had been made on the subject of contagion; but he believed that even the Board of Health had modified its views, and now admitted that cholera was, at least in certain circumstances, contagious.

Dr. Baly said, that the author's hypothesis of the cause of cholera was not supported by the phenomena of the disease; for not only would the suppression of the cutaneous and pulmonary exhalations, and the transference of the fluids normally excreted by the lungs and skin to the intestinal mucous membrane, be inadequate to produce the enormous discharges which drain the system of its fluids; but it is a fact that the cutaneous exudation is, in many of the worst cases of cholera, greatly increased, a watery fluid being in these cases poured out by the whole surface of the skin, as well as by the mucous membrane of the intestines. The draining away of the watery part of the blood was certainly the main phenomenon of the disease, all, or nearly all, the others being secondary to and dependent on it. It undoubtedly was the cause of the coldness of the body; but it prevented the development of animal heat; not, he thought, by rendering the blood thick, and thus incapable of circulating quickly, as suggested by Dr. Snow, but by rendering impossible those chemical changes which normally go on in all parts of the capillary system, and which are the source of the animal heat. The circulation in cholera is undoubtedly much retarded; but the rapidity with which the nervous system of the patients is affected by chloroform, inhaled into the lungs, shows us that the motion of the blood would still be sufficient to allow of some evolution of caloric, if other necessary conditions were present; but an essential condition for the normal processes of nutrition, and for the chemical processes on which the temperature of the body is dependent, seems to be the presence of a certain proportion of water in the blood; and it is most probably because this is wanting, that the body becomes cold. To the same causes are obviously to be ascribed the arrest of the natural secretions of the liver and kidneys, and the depressed state of other organic functions of the body; and even the remarkable condition of the mind, and of the sensitive and motor functions—their state of comparative integrity—not only affords evidence that the nervous and muscular systems are, in a great measure, independent of the watery part of the blood, so essential to the organic functions, but is itself a result of the loss of that part of the circulating fluid. Under ordinary circumstances, if the respiration continued so imperfect, and the secretion of urine were suspended for a long period, as they are in the cold stage of cholera, the blood would become charged with carbonic acid and urea, and coma would result. In the cold stage of cholera, the natural chemical processes of the body are brought nearly to a stand-still, by the loss of the water of the blood, and neither urea nor carbonic acid is formed in large quantity; the powers of the mind and of voluntary motion are therefore left comparatively unimpaired.

Mr. W. F. Barlow could not discover how failures to demonstrate any poison in the atmosphere could lead to inferences of its non-existence. Probably it was so subtle, as to defy all scrutiny. Could the poison of small-pox or scarlatina be demonstrated in the atmosphere? Yet, was there any doubt that the air could be made infectious by, and could communicate, those diseases? He had paid some attention to the state of the muscles in cases of cholera. Nothing was more marked than the extreme excitability of the muscles during life, and their extraordinary manifestation of irritability after death. The facts which related to these points were most obvious; but as to the causes of them, there were so many difficulties in the way of satisfactorily accounting for them, that he begged the few words which he had to offer might be viewed as suggestive, and not positive. The condition of the blood must be considered, if we would even approach a rational conjecture on the state of the muscles. It seemed pretty clear, that there was a great defect in the formation of carbonic acid, in cases of cholera, and therefore in the production of animal heat. He had listened with interest to what had fallen from Dr. Baly, respecting the failure of chemical changes in the blood, and their consequences. There appeared a broad difference between death in the collapse of cholera and death by asphyxia, as ordinarily considered; in the

latter, noxious blood circulated, and impaired the irritability; but in the former, carbonic acid could not be carried freely through the system; if it were, coma would follow, and the heart's action would cease, even more quickly than it was observed to do. The elevation of temperature after death is very difficult to explain. It does not depend on the post-mortem muscular contractions, because extreme elevation of temperature has occurred when the contractions have not been met with. A formation of heat takes place sometimes after death: and in cases where the body was actually cold at the time of decease, the temperature has risen to 109° Fah. As a suggestion, Mr. Barlow threw out the idea, that this was caused by chemical changes going on in the blood after death, such as occur during life.

Dr. Basham did not collect from the paper, that the different salts were specified, and their proportions given; but the author inferred that the total saline ingredients was greater than in health. 8·56 parts in 1000 were given as the mean of several analyses; and this proportion was compared with Lecanu's standard of six parts in 1000, as the mean of healthy blood. It was generally admitted that Lecanu's standard was below the mean; Simon had placed it as high as about eleven; and if the mean of many analyses of healthy blood by various chemists—Enderling, Marchand, Becquerel, Simon, and our own English chemists—was taken, the average proportion of salines in 1000 parts would not be below 11·5 to 12. He had never found the saline matters of healthy blood, on an average, lower than 10 to 10·50. Individual cases would occasionally present a lower proportion. In two cases of cholera blood, examined last August, the saline matters were considerably below the mean; this was also the case with regard to the fibrin, and these results agreed with Simon's analysis. Dr. Thomson estimated this element above the average, but did not state whether this increase was absolute, or only relative. The deficiency of water in 1000 parts would cause an apparent increase in the whole solid contents; but was the fibrin augmented in relation to the amount of albumen and blood-corpuscles? He (Dr. Basham) was surprised that no reference was made to the presence or absence of urea in the serum of the blood; he had obtained it, in several instances, during the late epidemic, from the blood, both during life and after death. The statement, that there was no departure from the natural standard of the urine in a small quantity taken from the bladder, was very contrary to the results of numerous examinations of the urine, made during the prevalence of the disease. In every case, the fluid found in the bladder was more like an alkaline solution of albumen than urine; it contained no urea, no phosphatic salts, traces only of chlorides; and these qualities were observed even in the urine of those who, for many days after the collapsed stage, passed scanty urine, but who ultimately recovered.

Dr. Copland regarded the heat of the body after death as the result of incipient decomposition. In all cases of death from cholera, the stomach and intestines were easily torn. This was also observed in yellow fever, and in malignant puerperal fever. In these diseases, there was a great increase of temperature after death.

WESTMINSTER MEDICAL SOCIETY.

Saturday, January 26, 1850.

Francis Hird, Esq., President, in the Chair.

Caries of the Head of the Femur. Mr. Henry Smith exhibited two specimens, both of which had been excised during life. The first had been removed by Mr. Morris, of Spalding, from a youth, eighteen years of age. This instance was eminently successful.[1] The other specimen was

[1] This case has been published by Mr. Morris in the *Provincial Medical and Surgical Journal* for February 6, p. 57, as well as in other periodicals.

obtained by him (Mr. S.) from an adult, between thirty and forty years of age, formerly in the Grenadier Guards. He had removed the diseased bone two years ago. The patient survived the operation six months. This last-named specimen, which had been macerated in warm water for three months, exhibited the early stage of caries; the disease had only existed eighteen months; the globular shape of the head was unchanged, but it was carious (honey-combed) all over. The neck of the bone was altogether unaffected; it remained of the usual length, and at the ordinary oblique angle with the head. In the other case, the disease had been in existence six years, and the head of the bone was completely flattened out, the neck being almost destroyed. Instead of an oblique arch, it presented a right angle with the head. In his own case, all the symptoms of dislocation of the head of the femur existed, and the operation proved his diagnosis to be correct. In Mr. Morris' case, it was supposed that dislocation had taken place, but this was proved not to be the case, when the operation was performed. A difficulty was experienced in dislocating the head, owing to a small piece of bone, which had separated from it, and lay in one of the sinuses. It was this which caused the symptoms simulating dislocation. There was very great shortening of the limb, in consequence of the destruction of the neck of the bones. He had often been asked, as to the manner in which locomotion could be performed without a hip-joint: but when the head of the femur was excised, a new joint might soon be formed. After the removal of the diseased bone, inflammation set in, lymph was thrown out in the acetabulum, (if healthy) the head of the femur became rounded off, and fibrous tissue was formed, from which resulted an entirely new capsule. Mr. Morris' patient was able to flex the mutilated thigh on the pelvis, as readily as the other, seven weeks after the operation. He had seen, a few days since, a woman who had been operated on by Mr. Fergusson, a year ago; she was able to walk, with the aid of a high-heeled shoe, without a crutch.

Malignant Disease of both Ovaries, the Rectum, etc. Mr. Nunn exhibited the morbid parts. The patient was aged sixty-two. She died in St. Giles's Infirmary, after several copious discharges of blood from the rectum. The right ovary presented the greatest evidences of malignancy. The left contained within it several cysts, the fluid in each of which differed in its appearance from that in the others. The gorged cells, which are said to be proper to ovarian fluid, were found in all, in greater or less abundance. The right ovary was highest in the pelvis, and was most plentifully supplied with blood. The spermatic artery entering its upper part, was excessively tortuous. In addition to this, branches from the right colic, superior and middle hæmorrhoidal, epigastric, internal iliac, and uterine arteries, also assisted to feed the tumour; the ureter was involved in the pedicle of this ovary. The uterus was dragged from the centre to the side of the pelvis, and was so placed, that its long axis was directed transversely, instead of being in its normal relations. The organ presented, on being laid open, no marks of disease, although malformed, by being divided into an upper and lower compartment. The os uteri was perfectly healthy, and had the appearance of belonging to a virgin uterus. The vagina and bladder were quite sound; the rectum, about an inch and a half from its lower termination, was perforated by a circular opening, large enough to admit three fingers, otherwise this viscus was healthy. The aperture formed the means of communication between the rectum and a highly vascular cancerous mass, situated in front of the rectum, and behind the vagina and uterus. This mass, if it originated in either of the organs referred to, must have occurred in the outer covering, since the mucous lining of all was, with the exception of the aperture mentioned, as sound as it is ever found in persons of an advanced age. The cæcum was thrown from its seat in the right iliac fossa, into the middle of the belly,—not by being displaced by the enlarged ovary,

but by means of the tension of the peritonæum. Cancerous deposit was found in the mammæ, and in several other organs.

Detached Osteophytes in the Longitudinal Sinus, and on the Surface of the Brain, of a supposed Epileptic. Dr. Woodfall exhibited two Osteophytes, taken from the longitudinal sinus of a woman aged sixty. They were of irregular shape, and each about an inch in length, and one-sixth of an inch in thickness at the thickest part. The larger weighed above two grains, and the smaller rather less than two grains. Dr. Woodfall only once saw the deceased in life, when she was in a state of stupor. He had ascertained from her sister, that she had, since her 14th year, been subject to fits, rather resembling syncope than epilepsy, but also somewhat epileptiform.

At eight o'clock on the morning of Oct. 20th, 1849, she was found on the floor of her bed-room, where she had fallen while in the act of washing herself, unable to rise, but perfectly sensible. Her speech was observed to be altered. She continued sensible until about eleven o'clock, A.M., when she gradually became unconscious; and when seen by Dr. Woodfall, at four o'clock, P.M., was in a state of profound coma. She died at three o'clock the following morning, nineteen hours from the commencement of the attack. The head was examined thirty-six hours after death. The substance of the brain was wounded in removing the calvarium, and a considerable quantity of blood flowed out. On the surface of the left hemisphere two thin plates of bone were found, of small size; and in the superior longitudinal sinus were exhibited the two others. The substance of the brain was generally soft. The floors of the lateral ventricles were stained with blood, which had filled them, but which had mostly escaped in opening the cranium. The softening was most marked in the posterior corner of the right ventricle, which, in parts, presented almost the appearance of pus.

Malignant Disease of the Bladder. By Dr. Lankester. In 1845, Dr. Lankester was consulted by a gentleman, sixty-two years of age, a small man, of active habits, and temperate all his life. He had, nevertheless, suffered much from dyspepsia, and had frequently had medical advice for his dyspeptic attacks. About twelve months before Dr. L. saw him, when working in his garden, he was attacked suddenly with pain in his back, followed by a desire to make water, which he found coloured with blood. He immediately sought medical advice, but the pain in the back continued, with a fresh appearance of blood in the urine. He wasted, lost his appetite, became sleepless, the frequent desire to pass his water increased, and the pain in the back continued, with great pain in the inside of the thighs. Under these circumstances, it occurred to his medical attendants that stone in the bladder was the cause of his symptoms; he was accordingly sounded more than once, but no stone could be found.

When first seen by Dr. Lankester, he was very thin, with an anxious countenance, and a peculiar stoop in his gait. He suffered much from dyspeptic symptoms, and after taking food he frequently had severe gastrodynia. The bowels were habitually constipated, and never relieved without medicine. He suffered great pain in the back; but referred the seat of it to the region of the bladder in front, above the pubes. Pressure in this region evidently gave him pain. The urine, which he passed every hour or hour and a half, was sometimes more coloured than at others. It gave an acid re-action to test paper; and, after standing a considerable time, threw down a deposit which, under the microscope, presented lithate of ammonia and organic shreds, apparently of mucus and fibrine. Blood and pus-globules could also be plainly distinguished. On applying heat and nitric acid, albumen was precipitated. With these symptoms, it was somewhat difficult to come to a conclusion with regard to the disease. Dr. L. was inclined to regard the case as one of chronic inflammation of the bladder, with ulceration. With the view of alleviating

the pain, opium and henbane were given, and mustard cataplasms applied over the region of the pubes. He seemed to get a little better under this treatment. He subsequently took copaiba. In the summer, he went into the country; and Dr. L. lost sight of him for a few months, till sent for to see him on the 21st of December. In the mean time, he had consulted an eminent surgeon, who had passed a catheter, and endeavoured to inject the bladder. This operation had been attended with great pain, and an increased amount of blood in the urine. In a note from the surgeon, who was then consulted, he says, "I could not, when I once tried to inject the bladder with warm water, get the fluid to pass, although the catheter was fairly beyond all possibility of prostatic obstruction." The patient was now in a much worse condition than when Dr. L. last saw him. He was exceedingly emaciated; the pain in the back and region of the bladder was constant and intense. He passed his urine more frequently than ever. It was occasionally bloody, but sometimes clear. On standing for a short time, it deposited a thick sediment, which consisted of lithates, mixed with amorphous organic matter. Nitric acid and heat showed a large quantity of albumen. His bowels were very much relaxed, and he was troubled with vomiting. The purging was arrested in the course of a few days, but the sickness continued. He took quarter-grain doses of morphia thrice a-day, which in some measure subdued the pain; but he gradually got weaker, and sank on the 2nd of January following.

Post-mortem Examination. The lungs and liver were healthy. The heart was of natural size; slight deposits were observed upon the mitral and semilunar valves. Both kidneys were enlarged, but the left much more than the right. The capsule was easily separable from the mass of the kidney. The tissue of the whole kidney was soft and easily broken down, but there was no conversion of tissue. The pelvis contained some puriform fluid, which was traceable down the ureter. Parts of the kidney presented granular degeneration. The coats of the bladder were firm, when grasped from the outside. It contained no urine. On opening it through the urethra, a loose organized mass of a yellowish colour was found free. Attached to a point near the neck of the bladder, was a granulated mass, highly injected, presenting portions of a red colour passing off into white. It was easily broken down with the knife, and yielded to slight pressure. Higher up the bladder, on one side of the fundus, another tumour presented itself, of larger size, but having the same character. The loose mass had evidently been separated from one of the portions still in connexion with the bladder. Dr. Lankester had not had the opportunity of examining these growths by the microscope; but no doubt could exist of their belonging to that variety of carcinomatous growths which have been designated medullary sarcoma, fungus hæmatodes, etc. The coats of the bladder were very much thickened. The prostate was considerably enlarged; and, on its being sliced, several points presented matter precisely similar to the growths in the bladder. The glands of the abdomen, the spleen, pancreas, stomach, etc., presented no trace of malignant disease.

Remarks. In cases of this kind, the general symptoms are often insufficient to afford a clear diagnosis. There are, however, some points with regard to the blood and other products in the urine, which may serve to assist our diagnosis. When blood flows after the introduction of the sound, or passes in small quantity from the urethra after the flow of urine has terminated, we may conclude that the hæmorrhage is not from the kidneys, but from the bladder. Sir Benjamin Brodie states, that it is not uncommon for small portions of the fungoid growth to be passed with the urine, which will discover the disease. This, however, does not always occur; and it was not observed in the case now related. It thus appears, that thus far we are without any absolute diagnostic of the presence of this disease. There is still, however, one mode of inquiry,—the microscope. At the time this case occurred to Dr. Lankester, we were not so well acquainted with the microscopical appearances of carcinoma as at present; and it appeared to him that the use of this instru-

ment afforded the most available means for diagnosis in this disease. Carcinoma in the bladder is generally of the softer kind, and as this organ is seldom affected by analogous formations, which occur on the external parts of the body, the diagnosis of malignant disease of the bladder is more difficult. Injections of warm water gave no relief in this case. Opium, as in so many other forms of malignant disease, seems to be the medicine which best alleviates the sufferings of the patient. Gallic acid, acetate of lead, etc., which act favourably in hæmorrhage generally, would probably be of service where the hæmorrhage was alarming in fungoid disease of the bladder.

Mr. Coulson agreed with Dr. Lankester, as to the difficulty of distinguishing morbid growths in the bladder. There was first the simple polypus, which might be of small size, or might occupy the entire cavity. There was, also, fungus hæmatodes, a very rare disease, and generally complicated with calculus. A man who had been well known about town, suffered from this disease, and had also numerous calculi. He sank at last from exhaustion. There was also the disease which was met with in Dr. Lankester's case, which he believed was medullary sarcoma. Mr. Coulson also mentioned the case of a gouty subject, who was suddenly seized with hæmorrhage from the bladder, complaining also of a wearing, aching pain in the loins. There was afterwards, almost invariably, blood in the urine, at first diffused through the fluid, and passed without pain, and subsequently in the form of coagula, with great pain. The discharge of blood suddenly ceased ; and he was then affected with the symptoms of chronic inflammation of the bladder, with indications of calculus. No stone could be discovered. There was great difficulty in passing the catheter, and moving it round the bladder ; while doing this, a portion of morbid growth came away, which proved to be malignant. He was not sounded afterwards. The use of the catheter was attended with exquisite pain. The patient died in twelve months, worn out by intense suffering. For ten or twelve days before death, the urine escaped involuntarily and without pain. The abdomen only was examined. The bladder and surrounding parts filled up the entire pelvis. The former was so soft, that it gave way in taking it out ; it was filled with a mass resembling cauliflower excrescence of the uterus. It appeared to have arisen from the mucous and sub-mucous tissue of the organ.

Malignant disease of the bladder is only relievable by opiates ; and the less done, the better for the patient. The diagnosis in this case was only obtained by bringing away a portion of the diseased growth. The microscope might certainly be of use by detecting the existence of nucleated cells ; otherwise there are no positive signs by which a malignant disease affecting the bladder can be detected before death, except from a fortuitous occurrence, such as was met with in his case.

The President directed attention to the means of arresting hæmorrhage from the bladder. A patient, under M. Dupasquier's care, an old man of seventy, passed daily a pint of blood for a long while, and used many medicines, including gallic acid in large doses, and the muriated tincture of iron, but without benefit. No disease of the bladder could be detected. Whether it were the effect of the drug, or of accident, no one could tell ; but the hæmorrhage ceased while he was taking the oxide of silver.

Mr. Nunn stated, that in a case of Mr. Simon's, of superficial carcinoma of the prostate, the microscope was employed, the malignant character of the disease discovered, and the catheter, in consequence, was not again passed. Sir B. Brodie had seen the case, and remarked that it was the first in which malignant disease of the urinary organs had been detected during life.

Mr. Kesteven mentioned a case of hæmorrhage from the bladder, which he published some time since in the *Medical Gazette*. The diagnosis of malignant disease was not clearly made out ; and some thought there was gouty disease, others stone. The tincture of the sesqui-chloride of iron was of most service. It was looked upon as malignant, because the hæmorrhage continued while the patient was at rest.

Mr. Hancock could only see a very great impropriety in sounding the bladder in a case of hæmorrhage from that organ, unless there were symptoms of stone, which in malignant disease of the bladder was not usual.

Dr. Sibson thought the microscope of more use in discovering the source of the hæmorrhage, than the presence of malignant cells. If it came from the coats of the bladder, it would not present the tubular casts from the kidneys. The source being discovered, would serve as a guide to the treatment. To relieve pain, he advised belladonna, or stramonium. Belladonna completely controlled pain; it was not necessary to give it as often as opium, as its action was more permanent; it required, however, to be carefully watched. It would be useful to give opium with it, as the combination of narcotics is more powerful than a large dose of any one singly.

Dr. Daniell, after making some remarks on the diagnosis, said that turpentine in small doses will often arrest renal and vesical hæmorrhage.

Mr. Gower said, that he had considerably relieved pain in two cases, very similar to those described by Dr. Lankester, by giving gum-arabic. The patients had consumed about a quarter of a pound weekly.

Dr. Lankester thought that medullary sarcoma and fungus hæmatodes were transition diseases, and still considered his case one of fungus. The principal malignant diseases of the bladder are of a soft kind, in which the cells easily break down, and are found in the urine. The finding of nucleated cells is, however, not a diagnostic sign, as they are met with in fibrous and other tumours, interspersed among other cells; when, however, these cells are very numerous, then we may conclude that malignant disease exists. We cannot, however, affirm that their absence is a proof of the non-existence of malignant disease; because the diseased growth may not have become broken down.

Saturday, February 2, 1850.

Francis Hird, Esq., President, in the Chair.

Caries of the Head of the Femur. Mr. Haynes Walton exhibited three specimens of Caries of the head of the Femur, removed during life, to show the alteration in form and structure, effected by morbus coxarius. The first was taken from a girl nine years old. In place of the head and neck, there was a rounded and spongy portion of bone, projecting little more than half an inch from the femur. She made a good recovery. The *second* was from a lad twenty years old. The head was nearly gone; the neck was of the natural size, but so soft that it broke in two during the operation. The *third* was from a boy eleven years old. Part of the head was lost, and the neck was shortened and reduced in size. Mr. Walton was anxious to discover some diagnostic signs by which dislocation could be ascertained. The change in the form of the bones he had shown, would cause all the symptoms of dislocation. The limb would be shortened and the trochanter elevated, and brought nearer the pelvis. From the position of the limb, nothing could be learned; it may be turned inwards or outwards; and the thigh may be flexed on the pelvis or extended.

Passage of a Plum-stone, several months after it had been swallowed, through a Fistula at the Umbilicus. Mr. G. B. Childs exhibited a plum-stone, which had escaped from a fistulous opening at the Umbilicus, after having been lodged in the intestines for five or six months. The patient, a boy four years old, had suffered from hooping-cough, measles, and scarlet-fever. The latter had been followed by dropsy, from which he recovered, and seemed to enjoy good health afterwards. Some months since, the umbilicus became swollen and red, and an abscess formed, which burst, and continued to discharge for several months. He was admitted into a hospital, where the opening was enlarged, and another made about two inches below it. He remained there a fortnight; and while in the hospital, feculent discharge oc-

curred from the lower aperture. When he came under Mr. C.'s care, there were two sinuses—one in the centre of the umbilicus, the other a little lower down: the latter yielded the fæcal discharge. On the 16th of last month (January), the plum-stone was discharged from the wound, after great suffering. After this, the fæcal discharge was greater; and, on probing the wound, the instrument passed at least two inches backwards into the abdomen. The bowels acted freely, and the general health was pretty good. Did the abscess and fæcal fistula in this case arise from the presence of the foreign body in the intestine, or from the previous ill-health of the patient?

On Inflammation of the Mammæ. By Mr. T. W. Nunn. After a few remarks on the common modes of treatment of Inflammation of the Mammary Gland, and their inadequacy to prevent suppuration, the author stated that he proposed the following plan. 1. To confine the patient to the horizontal position. 2. To prevent any extraneous irritation of the inflamed organ. 3. To envelope the breast with mercurial ointment spread on thin linen, and to cover this with a tepid poultice. 4. To support the gland by a suitable bandage, when the horizontal position cannot be maintained. 5. After the constitutional irritation has been allayed by a brisk purgative, effervescing salines, and proper regimen, to carefully watch the state of the pulse, and look for the proper moment for the administration of tonic medicines. 6. Wine and stimulating articles of diet should be allowed only with great caution. In the majority of cases, Mr. Nunn was of opinion that, after the first day or two, the patient needs bark and ammonia, quinine and iron, rather than depletive drugs. The strength of the mercurial application should be adapted to the condition and natural texture of the skin covering the gland. In some instances, the unguentum hydrargyri fortius will not be found too powerful; in others, it will be necessary to dilute it with an equal proportion of ceratum resinæ. A combination of the extract of belladonna, hyoscyamus, or opium, in the proportion of ʒi. to the ʒi. of ointment, will be most effectual in allaying the intense agony frequently complained of. The leading idea in treating mammary inflammation, should be the prevention of suppuration: when that cannot be avoided, the attempt should be made to render it as circumscribed as possible. Mr. Nunn opposed large incisions of mammary abscesses, and thought the practice of laying open extensive sinuses of the breast uncalled for. The tissue of the gland, he averred, should not be cut; and he quoted Dr. Gibson of Philadelphia, to show that sinuses may be obliterated by pressure. The importance of preserving the integrity of the gland, he (Mr. Nunn) observed, cannot be overrated. He related several cases in illustration of his position.

Case i. A young woman, aged twenty-two, who had been confined with her second child about a month, applied to Mr. Nunn, with her breast in a state of intense inflammation. She had been applying poultices and hot fomentations for two or three days, without any relief. Her tongue was white; bowels confined; much thirst; pulse rapid, but weak; skin moist. She was ordered a saline aperient of magnesia, and a drachm of decoction of cinchona twice a day; also to have mercurial ointment, diluted with resin cerate, applied to the breast. In ten days, the redness, heat, and swelling, had much diminished; and the constitutional symptoms were improved. The ointment gave great ease, and had enabled her to sleep; it had not produced any unpleasant effects. She was ordered to apply the ointment more sparingly, and to take quinine and porter. In a few weeks, the disappearance of some hardening which had remained, indicated a satisfactory termination of the case.

Case ii. M. R., aged thirty-nine, had chronic inflammation of the right breast, for which she had been subjected to a variety of treatment. The swelling was principally confined to the lower half of the organ; there was little redness, but much tenderness. Mr. Nunn ordered mercurial ointment, with a drachm of opium to the ounce, to be rubbed in; and the patient to take

ammonia, with cinchona and conium. The ointment gave rapid relief, and the patient soon recovered.

Case III. M. M., aged twenty-one, applied to Mr. Nunn, about six weeks after delivery, with an acutely inflamed breast. The mercurial application was ordered. She neglected to report herself at the time appointed, but applied the ointment for a week. The system became affected: but no actual mischief appeared to have been done. This was the only case in which the mercurial application had effected more than was desirable; and though suggesting caution in its employment, Mr. Nunn did not consider that it formed an objection.

Case IV. M. A., aged forty, had mammary abscess, which had been treated by poultices, etc. It had burst, and left a circular ulceration of the size of half-a-crown. The granulations on the surface of the gland were weak and flabby, and covered with a copious secretion of pale pus. Mr. Nunn discovered a large flat sinus, extending a considerable distance beyond the margin of the ulcer; the discharge from it was very abundant, and depressing to the patient. The poultices were discontinued; bark and ammonia were administered, with a more generous diet. The sinus healed without the necessity of laying it open, and the patient was restored to health.

The author pointed out the importance of attending to the peculiar relation in which the mammary gland stands to the economy, and to the effects produced by inflammation of the trabecular and of the secreting structures. It is important to the mother, as well as to the child, that the integrity of the organ should be maintained. Mercury, in virtue of its efficacy in checking the tendency of blood to deposit fibrin, and of its property of causing the reabsorption of effused lymph, seems to be strongly indicated in the disease under consideration. Its benefit is obvious, when we reflect that by the formation of an abscess in the substance of the gland, the ducts are destroyed, and the portion of the gland becomes isolated. For a time, it may give no inconvenience; but, under the excitement of lactation, it may become the seat of a fresh attack of inflammation. This, Mr. Nunn observed, must have been met with by every practitioner of experience. In conclusion, Mr. Nunn said that he would not be understood to advocate the use of mercury in all cases of mammary irritation; but he believed the plan he had recommended likely to prove of great service.

Mr. I. B. Brown said, that one great cause of inflammation of the breast was, the not applying the child to the breast early enough. This cannot be done too soon after delivery. Stagnation of the milk in the breast acts as an irritant, and causes inflammation. It can be overcome by general remedies, —the horizontal position, keeping the breast supported, causing the skin to act, and by warm-water dressing. He objected to drastic purges. Parturient women cannot bear to be lowered, and ought not to be subjected to the depressing action of mercury. Mammary inflammation was often caused by keeping the patients on gruel, when they required an improved diet. The excess of fluid was bad. Most patients subject to inflammation of the breast were of the strumous diathesis, and could not bear mercury, which would deteriorate their milk, and injure their offspring. Mercurial ointment might be useful in chronic mammary inflammation; but it could not be used with safety, he thought, in an acute attack.

Mr. Gay did not think the lacteal vessels were implicated in the disease; the abscess was situated in the cellular tissue; for, after recovery, the breast generally resumed its proper action. He was in favour of the early use of the lancet in opening the abscess, to prevent the burrowing of the matter, and the formation of sinuses, which are difficult to treat. The fascia, in some cases, becomes more dense, from the inflammatory action, and thus prevents the abscess from bursting through it.

Dr. King would be inclined to give Mr. Nunn's treatment a trial.

Mr. Greenhalgh had tried the plan, and found it successful.

Mr. Hancock thought that mercury would be useful in cases of occlusion of the lacteal duct, as it would be probably caused by effusion of lymph, and mercury is the best absorbent. His practice was, to keep the child to the breast as long as possible; and when that could not be continued, to draw off the milk artificially. Mr. Nunn's plan was not original; for he (Mr. H.) had used mercurial plaster to the breast for the last five or six years, with an opening in it for the discharge to pass away. He did not consider these cases as instances of phlegmonous, but of irritative inflammation. Hyoscyamus was of great service. He objected to the early opening of an abscess, unless there be great constitutional disturbance: when he did use the lancet, he avoided incising transversely to the ducts, so as not to wound them.

Dr. Murphy said, that no one plan of treatment could be followed as the best, in all cases of inflammation of the mammæ. Mammary abscess might be sthenic or asthenic. In strong, healthy young women, where the milk, from some cause, was not withdrawn, he was satisfied, that if proper antiphlogistic measures were adopted, there would be no reason to fear abscesses forming. He advised the combination of tartar emetic with the neutral salines. The feelings of the patient should be consulted, as to the use of warm or cold applications. In weak and irritable women, if purgatives and tartar emetic were used, the irritation would be increased, and abscess after abscess would form. Such patients require tonics and support. If, on the other hand, a robust, plethoric woman, be placed on full diet, after parturition, mammary abscess will be very apt to occur.

Dr. Manson stated, that at the General Lying-in-Hospital, it had been the custom for the last twenty years, when the breast became hard and knotty after parturition, to rub in freely the linimentum ammoniæ, as a preventive of suppuration; and so successful had this practice been, that they had scarcely ever had a case of mammary abscess.[1] The liniment is rubbed in for ten minutes or a quarter of an hour, until the breast becomes soft and supple. He thought it acted as an emollient and evaporating lotion, while the stimulant quality of the ammonia was evidenced in the reduction of the quantity of the milk. When the inflammation is so severe, that friction with the liniment cannot be used, he thought the mercurial ointment might be tried, with a poultice over it. In a very severe case, chloroform was given before friction was used, and successfully; the patient recovered without suppuration.

Mr. Coulson recommended the treatment of mammary inflammation by pressure; strips of soap plaster, encircling the breast, being applied on the same principle as in orchitis. He considered it eminently serviceable in the incipient stage, perhaps not so much so when the disease is more advanced, but again advised it after the abscesses have burst, and sinuses have formed. The advantage attending this plan is, that the patient can go about her avocations while under treatment.

Saturday, February 9, 1850.

Francis Hird, Esq., President, in the Chair.

Malignant Disease of the Ovaries. Mr. Greenhalgh exhibited a very interesting specimen; and stated, that the history of the case would be given to the Society on some early occasion.

Treatment of Stricture of the Urethra by Perineal Incision. Mr Henry Smith read a paper upon this subject. The principles of practice which he wished to establish, were three in number, viz:—

1. Strictures so obstinate as not to allow the passage of any instrument, and so complicated as to resist the ordinary modes of treatment, should be treated by free Perineal Incision.

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[1] A more common, and, so far as our experience goes, an equally successful method, is frequent gentle friction with warm olive or other bland oil. Editor.

2. Strictures which permit the passage of the smallest instrument, ought not to be cut, because it has been found that this operation is not unfrequently followed by fatal results.
3. Strictures of a bad kind, which do not admit of cure by simple dilatation, may often be treated successfully by the potassa fusa, which, being safer than incision, ought to be tried before that operation is resorted to.

The author dissented from, and strongly condemned the practice advocated by Mr. Syme, of freely dividing with the knife strictures which, though obstinate, permitted a bougie to enter.

The discussion on Mr. H. Smith's paper was deferred.

Auditors' Report. From this document, it appeared that the balance in hand was 73*l.* 18*s.* 3*d.*, after an expenditure of 92*l.* 15*s.* 2*d.* The total receipts during the year were 166*l.* 13*s.* 5*d.* The item 23*l.* 19*s.* 11*d.*, for printing the Abstract of the Proceedings, seemed almost an unnecessary waste of money, considering that the journals give much better accounts of memoirs and discussions.

Nomination of Candidates for Office. In accordance with the laws of the Society, the President intimated that any Fellow might propose those whom he thought most eligible. The offices to become vacant, were those of a President, two Vice-Presidents, and five Members of Council. Dr. Murphy was nominated as President, and Dr. A. B. Garrod and Mr. James F. Clark, as Vice-Presidents. For the five vacancies in the Council, numerous Fellows were proposed; but several having withdrawn, they were ultimately reduced to nine, viz:—Dr. Sibson, Dr. Cormack, Dr. Tilt, Dr. Tyler Smith, Mr. Hird, (retiring President) Mr. B. Travers, jun., Mr. Greenhalgh, Dr. Lightfoot, and Mr. H. Walton.

Union of the Society with the London Medical Society. The President gave a detailed account of the negociations now taking place between the Councils of the two bodies. The proposed amalgamation has already been noticed by us (p. 214); but this was the first occasion on which it was formally announced to the Fellows.

Saturday, February 16, 1850.

Francis Hird, Esq., President, in the Chair.

Chloroform in Abortion. Mr. I. B. Brown mentioned a case of abortion at the sixth week, in which, by subjecting the patient to the influence of Chloroform, he was enabled to remove the ovum, previously grasped by the os uteri; the hæmorrhage ceased. Mr. Brown thought that the treatment which he had adopted in this case, would be found generally applicable to others of like nature.

Dr. Tyler Smith had never seen a case of abortion in the early months, in which he could not get away the ovum by means of an enema, containing some stimulating ingredient, such as turpentine. He had seen some remarkable cases in which this treatment had been most successful. In one case which he had attended, with Dr. Cormack, a lady's life had been saved, when at the last extremity, by this proceeding. A stimulating enema invariably produced reflex dilatation of the os uteri, and contraction of the uterus itself.

Dr. Cormack, in reference to the case mentioned by Dr. Tyler Smith, said, that he was quite satisfied that the turpentine enema had saved the patient from impending death. He greatly approved of the practice advocated by Dr. Tyler Smith; and another illustration of its value was afforded by the case which he had detailed to the Society on the 19th January. [*Vide* London Journal of Medicine *for February*, p. 203.]

Adjourned Discussion on Mr. H. Smith's Paper on Stricture. The discussion was animated and interesting; but as all the points adverted to have, in one form or another, been recently laid before the profession, it is not necessary to give a detailed report of the speeches.

Dr. Knox, (late teacher of anatomy in Edinburgh,) in a lively address, severely criticised Mr. Syme's recent work on Stricture. He thought that a verdict on his practice could not yet be given, as time might yet show that the cases boasted of as cures, were, after all, not cures; it was not temporary amendment, but permanent relief, which was entitled to the name of a cure. Some surgeons were too quick—too quick—in publishing their successful cases.

Mr. Wade gave an exposition of his views regarding the use of potassa fusa, and replied to certain passages in Mr. Syme's recent work.

Mr. H. Walton, Mr. G. B. Childs, and Mr. Acton also addressed the Society.

Election of Office-Bearers to fill the Places of those retiring in rotation. At 10 o'clock the ballot closed, and the following was announced as the result:—President—Professor Murphy: Vice-Presidents—A. B. Garrod, M.D.Lond., and J. F. Clarke, Esq.: Councillors—Francis Hird, Esq., Francis Sibson, M.D.Lond., F.R.S., Benjamin Travers, Esq., jun., John Rose Cormack, M.D.Edin., F.R.S.E., R. Greenhalgh, Esq. The above are the *new* office-bearers; and along with the following of the old list, constitute the Executive Council:—Vice-Presidents—E. Lankester, M.D., F.R.S., J. F. Marson, Esq.: Treasurer—Augustus Sayer, M.D.: Honorary Secretaries—S. W. J. Merriman, M.D., C. H. F. Routh, M.D.Lond.: Councillors—W. Harding, Esq., F. R. Manson, M.D.Lond., T. H. Tanner, M.D., W. R. Rogers, M.D., W. Harvey, Esq.

The Retiring President, Mr. Hird, having left the chair, Dr. Chowne proposed that Dr. Murphy should take it in capacity of Vice-President, that advantage might be taken of the crowded meeting, to mark the high appreciation which the Society had of the eminent services rendered by Mr. Hird, during his time of office, which would terminate at next meeting, when the minutes of the present had been confirmed. The proposition was carried by acclamation; as was likewise a formal vote of thanks to Mr. Hird, to which that gentleman replied in a few elegant and hearty sentences. At the conclusion of his speech, the Fellows rose *en masse*, and maintained, for several minutes, the most enthusiastic cheering.

MISCELLANEOUS INTELLIGENCE.

Royal Medical and Chirurgical Society. For some days past, it has been currently rumoured, that at the Election of Office-bearers on the 1st March, an attempt will be made to modify the House-list in some of its leading features. The extent to which this endeavour may be supported, will probably form but a feeble indication of the degree of dissatisfaction which prevails; as, though the majority of the Society is notoriously adverse to the present system of management and distribution of honours, yet to obtain an amount of co-operation sufficient to cast out any one of the names published by the council, can hardly be expected. To accomplish this, more than a hurried organization is required. Whatever be the result of the ballot, and whatever be the character of the discussion which may precede or follow it, we sincerely hope it may conduce to the permanent good of an Institution, which contains within it all the elements of greatness and utility; and that from this day henceforth there may be secured an entire community of feelings and interests between the ruling and the other Fellows Antagonism between the rulers and the ruled, may be a normal condition in Royal Colleges founded in, and for, a by-gone age; but it is neither necessary nor natural in an institution, with so free a constitution as the Royal Medical and

Chirurgical Society. As we said on a former occasion, (p. 178,) the most needed reform is to abolish the noxious and illegal bye-law, which gives birth to the House-list: and let there be *an open nomination by the Fellows*, the ballot determining the election, as appointed in the charter, and not being a mere farce, or necessitating a troublesome concert, when others than those named by the Council require to be brought forward. There can be little prosperity, or brotherly feeling, in the Society, till the President and Council are the free choice of the Fellows, and the selection of papers for publication be impartial.

The Royal College of Surgeons of England is at present said to be in a peculiar position with regard to its treasury, and to the increased probability of a rival College being sanctioned by Government. This measure, it is to be hoped, may not be required; as, if an independent College examine in Medicine, Surgery, and Midwifery, the General Practitioners, who now in legions join the College of Surgeons—only (as they say) to be repudiated—would for the future enrol themselves in the new College, by which they would be fostered, and in which they might rise to professional distinction, apart from seniority or favour. There is no need of new Colleges, if the old ones were adapted to the age we live in, and to the more general diffusion of sound professional knowlege. As the general practitioners are every day becoming a more educated and scientific body, they can no longer be treated as their predecessors, the druggists, of 1815. Time, perhaps, may yet adjust all questions of Medical Reform; but, in the present transition state of the profession, it is incumbent on all to study conciliation, and, if need be, to make personal sacrifices for the common good.

Royal College of Surgeons of Edinburgh. A bill is now passing through the House of Commons, entitled—"A Bill for enabling Her Majesty to grant a new Charter to the Royal College of Surgeons of Edinburgh; and for conferring farther powers on said College." This measure relates, we believe, entirely to private arrangements, and not to public questions. It provides for bringing to a close the existing Widows' Fund, and separating the College from the Incorporated Trades of Edinburgh.

The Faculty of Physicians and Surgeons of Glasgow, we are sorry to learn, are applying for a new Charter, and increased privileges, as a licensing body. One incorporation, with the licensing power for the United Kingdom, would be sufficient; but with a College of Physicians and Surgeons in Scotland, the Glasgow Faculty is more than superfluous. It might, with great propriety, be merged in, or united with, one or other of the Colleges in Edinburgh.

Royal Edinburgh Maternity Hospital. The annual meeting of the Governors and friends of this Hospital was recently held,—the Lord Provost in the Chair.

Mr. Buchanan, S.S.C., the Secretary, read the Report of the Directors, which stated that, owing to a falling off in the amount of subscriptions, they had been under the necessity of drawing on the funds in the Bank to the extent of £60. During the past year, 687 persons had had medical attendance during their confinement; and 259 had, in addition, received shelter and maintenance within the Hospital. The expenditure amounted to £385. Two deaths had occurred during the year.

Bishop Terrot moved a resolution, to the effect that the Report be adopted and circulated, and recommending the charity to the more liberal support of the community; and suggesting that, since Edinburgh had become so distinguished in the obstetric art, there should be something more definite than a simple recommendation for increasing the subscriptions, by bringing the Institution under the notice of those strangers who were now continually resorting to Edinburgh. Dean of Guild Wilson seconded the motion, which was unanimously agreed to.—[*From an Edinburgh Newspaper.*]

Meeting of the Conference respecting Medical Reform. A meeting of the delegates of the various medical associations appointed to watch over the Medical Reform Question, was held on Tuesday, February 19th, at the Hanover-square Rooms, Nathaniel Clifton, Esq., in the chair. The answer of the Council of the Royal College of Surgeons to the Council of the National Institute, was submitted to the meeting.

The Chairman said that this document was only another illustration of the shuffling conduct of the College Council. It was for the Conference to decide, whether they would put up with such treatment any longer.

Mr. Bottomley said that it was now quite clear that we must have a separate and independent college, possessing full powers in every respect. It would be quite absurd to attempt anything further with the Council of the College. He should move, that "after the receipt of the letter from the Council of the College of Surgeons, now read, it is inexpedient to make any further application to that Council."

Mr. Bowling seconded the resolution.

Dr. Webster (Dulwich) said that no one could now doubt what was the course to be pursued. With respect to the letter from the Council of the College of Surgeons, it was unkind, uncourteous, unfriendly, and unjust, and calculated to produce great dissatisfaction and angry feelings throughout the great body of the profession. They had gone to that Council, to entreat them to do justice to the great mass of the surgeons of the kingdom, and this was the answer! If the Council had agreed to all that the Conference required, it would have left them, after all, in an unpleasant position. The profession must be re-modelled, so that equal privileges should be granted to all who had passed an adequate examination. Many gentlemen in practice were not members of the College of Surgeons in London, but doctors of medicine, licentiates, and Fellows of other bodies; and these should be privileged to practice; they were received into the army and navy, and why not to other public offices? If they all united, there could be no doubt of their attaining their object. With respect to the assumption of the Council of the College of Surgeons, that they had the full control over the examination of surgeons, and were the only surgical body in the kingdom, he had to state that the Scottish Universities had the power, after examination, of granting a diploma of Magister Chirurgiæ, and he believed the English Universities had the same. The London University, at all events, by their Charter, had the power, and did examine, in "medicine, surgery, midwifery, and pharmacy," and he believed, could grant degrees in these separate departments. On what grounds, then, did the College of Surgeons assume their sole power over examinations in surgery? It was evidently a mere assumption. This was a precedent, at all events, with which they could go before Sir George Grey. (Cheers.)

The resolution of Mr. Bottomley having been put, was carried unanimously.

It was stated by Mr. Ancell and Mr. Bird, that the Council of the National Institute had, on the part of that body, answered the letter of the Council of the College of Surgeons: that letter would shortly be published. They might observe that the Council of the National Institute were now determined to obtain a new College, with full right to examine in all branches, and to be independent of all other bodies. That Council, wishing to act on the principle of mutual concession, had, at the suggestion of Sir George Grey, agreed to certain principles; but this was under very different circumstances from those in which they were now placed; and the consequence was, they must agitate for an independent College. (Great cheering.)

The following resolution was proposed, seconded, and unanimously carried:—

"That the resolution, empowering the deputation to wait upon Sir George Grey, passed at the last meeting of the Conference, be acted on; and that a letter be written to Sir George Grey, requesting him to appoint a time to receive the deputation."

Several gentlemen having expressed their determination to carry out the objects of the meeting, the Conference separated, after making arrangements for future action. [Abridged from the *Lancet*.]

Cholera Services. Testimonial to Mr. R. H. Whiteman, Surgeon, Putney. On the 16th of January, Mr. R. H. Whiteman was presented with a valuable testimonial, purchased by the subscriptions of numerous inhabitants of Putney. It consisted of a handsome silver tankard and salver, and a gold watch, upon each of which was engraved an inscription, indicating the sense entertained by the donors of the "zealous and efficient services rendered by Mr. Whiteman, as the medical officer of the district, during the late visitation of cholera." The presentation was prefaced by an address by the Rev. Edward East, the officiating minister of the parish: and Mr. Whiteman made a suitable reply.

Source of the Diplomas of London Physicians. A correspondent of the *Provincial Medical and Surgical Journal*, has given the following analysis of the sources of these honours of medical men, whose names appear in the London department of the *Medical Directory*. It will be observed, that 509 have a degree of M.D., and 45 that of M.B. They are thus distributed to the various universities.

Edinburgh	M.D.	178
London	M.D. 36, M.B. 30	66
Oxford	M.D. 24, M.B. 1	25
Cambridge	M.D. 38, M.B. 8	41
Dublin	M.D. 6, M.B. 5	11
St. Andrew's	M.D.	68
Glasgow	M.D.	35
Aberdeen	M.D.	30
Paris	M.D.	15
German	M.D.	59
Various Continental Universities—Austria, Prussia, Italy, etc.	M.D.	13
America and U.S.	M.D.	6
Nondescript (*i. e.*,) the degree being given without the University being mentioned	M.D. 6, M.B. 1	7
	Total	554

Provident Dispensaries. Mr. H. L. Smith, of Southam, proposes to renew the discussion of his favourite scheme, and has announced his intention of spending some months in London, that he may the more easily make known his opinions regarding, and his experience of, these admirable institutions, of which he may be said to be the father. We have reluctantly been obliged to defer the publication of Mr. Smith's lecture for another month.

Royal Society. The abolition of the Committees has given great satisfaction to the majority of the Fellows: and is likely to conduce to the prosperity of the Society.

APPOINTMENTS.

Cooper, White, Esq., appointed by the Queen, Senior Surgeon to the Artillery Company of London.

Freer, Dr., appointed Physician-Accoucheur and Lecturer on Midwifery in Middlesex Hospital, in the room of Dr. West, resigned.

Jeaffreson, Henry, M.D., appointed by the Queen, Physician to the Artillery Company of London.

Latham, Dr., elected President of the Pathological Society of London.

Law, Charles, Esq., appointed by the Queen, Surgeon to the Artillery Company.

Stewart, Dr. A. P., elected Assistant-Physician to the Middlesex Hospital, in the room of Dr. R. G. Latham, resigned.

Winslow, Dr. Forbes, Editor of the *Journal of Psychological Medicine*, elected a Fellow of the Royal College of Physicians of Edinburgh.

OBITUARY.

Buckle, Dickins, Esq., Deputy Inspector General of Hospitals, at Newport, Isle of Wight, aged 88, on 15th February.

Craven, Robert, Esq., F.R.C.S.E., Surgeon to the Hull General Infirmary, at Hull, on the 24th January.

Dixon, Thomas, Esq., many years Surgeon of the Hereford Regiment, at Park Place, Chelsea, aged 82, on 14th February.

Ferguson, Duncan, Esq., late House-Physician to King's College Hospital, at 32, Sidney Street, Brompton, after a severe attack of typhus, aged 26, on 1st February.

Fitton, Dr., drowned in the wreck of the *Richard Dart*, convict-ship.

Gale, Mr., Surgeon, drowned in the wreck of the *Richard Dart*, convict-ship.

Good, Samuel, Esq.. one of the Surgeons in Ordinary, to H.R.H. Prince Albert, and formerly Surgeon-Major to the Scots Fusilier Guards, at Worcester, on 22nd January.

Lowe, Richard, Esq., Senior-Surgeon to the Bristol Infirmary, after a severe illness, on the 9th February.

Oxley, Edward, M.D., at Exeter, of chronic bronchitis, aged 80, lately.

Reynolds, Cornwall, Esq., Surgeon R.N., at Mare Street, Hackney, on 13th Feb.

Swinson, George, Esq., Surgeon, at his residence, Church Lane, Handsworth, aged 85, on 11th February.

BOOKS RECEIVED.

Batchelder on Cholera. New York: 1849. Daniell (Dr. W. F.), Medical Topography and Diseases of Guinea. London: 1849. Instructions for Making Unfermented Bread. By a Physician. 16th Edition. London: 1849. Mackness on the Climate of Hastings. London: 1850. Miller (Dr. James) on the Kidney in Scarlatina. London: 1850. Tenth Annual Report of the Crichton Asylum. Dumfries; 1849. Transactions of the American Medical Association. Vol. II. Philadelphia: 1849.

ERRATUM.

In the February number, p. 139, line 19 from the bottom, for "*bee-hive*", read *bechic*. The latter term is sufficiently absurd; but, from the misprint, we have represented the author as writing absolute nonsense.

TO CORRESPONDENTS.

Unfermented Bread is wholesome: and, if common care be used, the fear of its containing arsenic need not exist, as we will show next month.